# Natural Science

## A Monthly Review of Scientific Progress

October 1899

## NOTES AND COMMENTS.

### The Scientific Spirit.

In his eloquent presidential address to the British Association at Dover, Sir Michael Foster raised an interesting question when he inquired into the characteristics of the scientific spirit. It was after reminding his audience of the great strides in natural knowledge since 1799, and of the resulting increase in man's mastery over nature, that he roused expectation by pausing to inquire whether all this has had any effect on the mind itself. The scientific spirit has been developed, he allowed, but what is this scientific spirit?

Surely the learned Professor must have thought that his audience could not stand much psychology, for his treatment of the interesting problem which he raised was easy-going. He pointed out that the features of the fruitful scientific mind were in the main three—truthfulness, alertness, and courage. To the objection that these qualities are not the peculiar attributes of the man of science, but may be recognised as belonging to almost every one who has commanded or deserved success, whatever may have been his walk in life, he answered that this was exactly what he wished to insist—that the men of science have no peculiar virtues, no special powers, being ordinary men with characters common, even commonplace. Science, as Huxley said, is organised common-sense, and men of science are common men, drilled in the ways of common-sense.

This may be true enough—and it speaks volumes for the candour and tolerance of the British Association that such plain-speaking was even applauded—but it was none the less an evasion of an interesting problem. What we wished was an analysis of the intellectual qualities of the scientific mood. It may have been useful to point out that science is not something *per se*, apart from other intellectual products, and that the scientific mood is germinal, at least, in most healthy people, but it would have been interesting if Sir Michael Foster—as one of the most scientific men in Britain—had told us what differentiates the mood expressed in, for instance, his "Text-Book

of Physiology" from that expressed in Green's "Prolegomena," or in Newman's "Sermons," or in Whistler's "Gentle Art," or in Meredith's "Ballads of the Earth." Altogether apart from subject-matter, the intellectual note of these is quite different from that which characterises the immortal text-book referred to, and what we wished was that the Professor had told us what his particularly well-marked differentiating feature—obscured by the word "scientific"—really meant.

Much more satisfactory was the concluding part of the address, in which the President discussed the solidarity and internationalism of science. "The man of science," he said, "cannot sit by himself in his own cave, weaving out results by his own efforts, unaided by others, heedless of what others have done or are doing. He is but a bit of a great system, a joint in a great machine, and he can only work aright when he is in due touch with his fellow-workers. If his labour is to be what it ought to be, and is to have the weight which it ought to have, he must know what is being done, not by himself, but by others, and by others not of his own land and speaking his tongue only, but also of other lands and other tongues." That this is being increasingly recognised is made evident in many ways—by international congresses and bibliographies, by international co-operation in great enterprises like the Antarctic Expedition, and in smaller endeavours like the production of *Natural Science*.

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## More Pleurococcus.

ANOTHER filament-forming Alga, to which its discoverer, Miss Snow (*Annals of Botany*, vol. xiii. No. 4, p. 189), has provisionally given the name *Pseudo-Pleurococcus*, has been separated from the aggregate of small unicellular green forms, so long known under the collective name of *Pleurococcus vulgaris*. The new form differs in the unicellular state from the true *Pleurococcus vulgaris*, which we are glad to see Miss Snow still recognises as a constant non-filament-forming species, by the possession of a pyrenoid and of a lateral aperture in the chloroplast, while it has the power of forming filaments when grown in certain nutritive solutions.

It appears also to be distinct from the filamentous form of *Pleurococcus* described by Chodat, in which the pyrenoid was absent, and which could not be distinguished in the unicellular state from the true *Pleurococcus*. In truth, the layer of green unicellular organisms so frequently met with on the bark of trees, etc., seems to consist, not of a single polymorphic species, but rather of a considerable number of real species, which may be isolated from one another only by the employment of certain modifications of the well-known methods of bacteriology, especially by rigid attention to the sterility of cultures.

## Asexual Nuclear Fusions.

FUSION of nuclei, whether it accompanies the union of so-called sexual cells, or occupies a position in the life-history which apparently denies it that dignity, must for some time remain a subject of absorbing interest, not only on account of its complexity, but also owing to the important biological questions involved.

Professor Percy Groom draws attention in a recent paper (*Trans. Bot. Soc. Edin.* 1898-99, pp. 132-144) to the number of such fusions of other than a distinctly sexual character, which we now know to occur in the vegetable kingdom.

Among fungi, in the Uredineae and Ustilagineae, the union takes place in the teleutospore, which, originally binucleate, contains but one nucleus at the period of germination, when it gives rise to the short sporidium-bearing promycelium. In Proto- and Autobasidiomycetes the fusion takes place in the homologue of the teleutospore, viz. the young basidium, which, when mature, represents, according to Brefeld, the Uredine promycelium, and bears basidiospores. Finally, in Ascomycetes the same phenomenon may be observed in the young ascus, which de Bary regards as a reduced sporophytic generation parasitic on the parent plant. Apart from fungi similar nuclear fusions are only known to occur among Angiosperms, where the union of two polar nuclei in the embryo sac precedes the formation of the endosperm, which, by the way, we are pleased to see the Professor regards as homologous with that of Gymnosperms, and consequently with the prothallus of the lower forms, its appearance having been postponed owing to functional degeneration. These fusions are thus always interpolations, and distinctly asexual in character, as is shown by the position they occupy in the life-history of such forms as the Ascomycete *Sphaerotheca* and the Angiosperms, in both of which the union takes place along with and subsequent to a well-marked sexual act, viz. the union of the antheridial and oogonial nuclei in the former, and that of the nuclei of the pollen-tube and egg-cell in the latter. In every known case they take place in a portion of the life-history, which has undergone degeneration, and which is at the same time fructificative in development, as well as frequently parasitic in character and sometimes at least homologous with the host plant (?).

Professor Groom suggests that if this fusion can be taken as evidence of vegetative degeneration in one segment of the life cycle, it may be possible to employ it as a means of distinguishing between antithetic and homologous alternation of generations among plants; but whatever be the physiological rationale of such fusions—and an adequate explanation seems still far to seek—they appear to have much in common with the similar phenomena which constantly accompany the union of sexual cells, and both will in all probability be ultimately found to perform similar functions in the life of the plant.

## Inheritance of Malformations.

THE inheritance of monstrous characters is a subject the examination of which may be expected to shed increased light on many important and still obscure questions, though it has hitherto failed to receive the attention it deserves. In a recent paper (*Revue Générale de Botanique*, April 1899, pp. 136-151) Hugo de Vries describes the results of a series of experiments, which he has for several years successfully carried on with regard to the inheritance of accidentally acquired fasciations in wild plants. By means of rigid selection and isolation of the parents, followed by careful cultivation of the offspring, he has been able not only to transmit the peculiarity through several generations, but even to increase the degree of fasciation. On the other hand, the tendency to reversion appears to be very strong, and notwithstanding the closest attention the resulting races never attain the permanency of those ornamental varieties so commonly cultivated in gardens. The plants examined were all facultative annuals, that is, species which are capable of giving rise to both annual and biennial individuals, and the differences between these are of some interest, if difficult of explanation. The annual forms, for example, never show fasciation till late in the season, and the malformation is confined to the upper part of the flowering stem, while those stems which spring in the second year from fasciated rosettes are fasciated throughout their whole length, and the malformation is more marked than in those of only annual duration, though even in these it may be considerably increased by early sowing under glass, or by any other method of cultivation which tends to increase the vigour of the young plant previous to the formation of flowering stems.

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## The Nucleolus in Heredity.

THE nucleolus has hitherto played with becoming dignity the somewhat passive part of a spectator in the nuclear quadrille, but Mr. H. H. Dixon (*Annals of Botany*, vol. xiii. June 1899, p. 269) has in these latter days dragged it from its inglorious repose, and it must now share the labours of the chromatin as a carrier of the hereditary substance. During division the chromosomes perform their accustomed task, but as soon as the cell enters a resting state the hereditary substance is divided between the newly formed nucleoli and the chromatic filament, the former taking the dormant idioblasts, which are not required for the functional development of the individual cell, while the remainder are left in the chromatin. On this hypothesis the apparent absence of the reducing division in higher plants is accounted for by supposing that the necessary elimination of excessive

germ-plasm is brought about by the extrusion of nucleoli, while the deficiency of chromatin, so often remarked in the nuclei of mature specialised cells, as compared with the large size of their nucleoli, would be a natural consequence of a reduction in the number of active, and an increase in the number of dormant, idioblasts, which might be expected to accompany specialisation if, as seems probable from the phenomena of vegetative regeneration, every mature cell must contain all the hereditary substance required for the development of an individual.

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## Inheritance of Longevity.

WALLACE, Weismann, and others have suggested that the normal length of life of organisms, which differs so much in different species, has been determined by natural selection. A creature lives as long as is good for the species. This was a general suggestion—prompted partly by the strange irregularity and apparent capriciousness of the length of life in different animals—and the preliminary question was not raised, "Is longevity a heritable character?" This is obviously a very important question, since natural selection could not determine or fix the fit duration of life unless that character were inherited. We are indebted to Miss Mary Beeton and Professor Karl Pearson for a contribution towards the required answer. In a paper entitled "A first study of the inheritance of longevity, and the selective death-rate in man," read before the Royal Society of London on 15th June, the authors show that directly and collaterally duration of life is certainly inherited in the male line in man. They believe this to be the first quantitative measure of the inheritance of life's duration. Further data for the inheritance of this character in the female line, and for the study of the inheritance of "brachybioty," or short-livedness as distinguished from longevity, are being collected. The inquiry should be interesting to actuaries as well as to biologists.

The second part of the paper is not less important. "In the presidential address at the Oxford meeting of the British Association we were told that no one had seen natural selection at work. In a criticism then published by one of us, it was suggested that every one who had examined a mortality table had seen natural selection at work. . . . All individuals die, but some, better suited by their constitution and characters to their environment than others, survive longer, and so are able, or better able, to reproduce themselves, and to protect for a longer time their offspring. To assert that natural selection does not exist, is to assert that the whole death-rate is non-selective, or is not a function of the constitution and characters of the individual. Looked at from this standpoint the existence of natural selection really becomes a truism. All that remains when we

desire to see it at work is to determine the relative amounts of the selective and non-selective parts of the death-rate for individuals living under the like environment. If, therefore, individuals living under much the same conditions are dealt with, the determination of the selective and non-selective death-rates is a measure of the quantitative amount of natural selection."

One method of dealing with the problem has been followed by Professor Weldon, who selected a certain structural part (in crabs), and sought to determine whether the death-rate is a function of the dimensions of this part. Another method has been followed by the authors. "We do not attempt to select any organ whatever, but select individuals having any general resemblance in their constitution, or in the whole complex of organs and characters, and correlate their fitness for surviving. Now relations or members of the same family are precisely such individuals. If there were no selective death-rate, there would be no correlation between the ages of death of, say, brothers. If there were no non-selective death-rate, we ought to find that the correlation between ages of death of brothers takes the value determined for the coefficient of heredity in brothers, *e.g.* the ·4 of stature, fore-arm, cephalic index, eye-colour, etc. Actually we find it to be something sensibly less than ·4. Our investigation shows that, in round numbers, about 80 per cent of the death-rate is selective in the case of mankind. To that extent natural selection is actually at work."

The authors close the abstract of their interesting preliminary paper with an appeal for biological experiment. "Various types of life ought to be submitted to ordeals of a kind like to those which occur in nature, and the correlation between the powers of resistance to these ordeals existing in members of the same family or brood determined. We shall thus be able to ascertain under a variety of circumstances the relative proportions of the selective and non-selective death-rates. . . . One may venture to express the hope that in a comparatively few years, if enough workers can be found for the experimental side of the subject, we shall no longer hear natural selection spoken of as hypothetical, but rather its quantitative measure given for various organisms under divers environments."

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## A Verbose Vitalist.

NATURALISTS of an earlier day would probably be surprised—if not shocked—at some of the contents of modern biological journals. We refer to the now frequent occurrence of pages thickly strewn with equations and mathematical symbols, of others bristling with "categories" and "principles," of others where the author seems at first to be living in

another world peopled by strange creatures called biophors and determinants, and worse. These things do not of course surprise or shock us, for we have realised the value of the statistical study of variations, the need of keeping on good terms with philosophy, and that Weismann's symbols are "not mere fanciful images, but realities," as he says, "in the same sense in which chemical atoms and molecules are realities." We are not surprised at these papers; what surprises us is that so few people seem to read them. A fragment of skin from a Patagonian cave seems to excite more interest than one of Karl Pearson's mathematical contributions to the study of evolution; the problem of tritubercuły is familiar, but Mr. Sandeman's "Problems of Biology" remains unheeded; discussions of mimicry abound, but we might almost count on our fingers the English references to Weismann's essay on Germinal Selection. Is it that we have forgotten our mathematics, is it that we have become after many lessons "philosophie-scheu," or is it that our love of the concrete is too strong? There are these and other reasons on our side, but it must be allowed that the fault is not wholly ours. It is certain that one reason why contributions to the philosophy of biology are so frequently disregarded, is the author's low standard of lucidity. Enigmatical sentences, tense with meaning, may be gloated over if they are written by Browning, but not if they come from a biologist. Aphorisms which sound as if they meant much (as they probably do), which seem, however, only successful in keeping their meaning hidden, may be entertaining in a novel by Meredith, but they are only irritating in an essay on morphogenesis. Thus, through the carelessness of authors and the busy preoccupation of readers, we are left to continue our work but slightly influenced by the constantly growing mass of occult biological literature. We know of a prominent worker who bundled up one of these voluminous riddles, labelled it "Davidson's Secret," and threw it on the top shelf; and we quite sympathise with any busy biologist who should similarly treat the little book before us. It is called "Die Lokalisation morphogenetischer Vorgänge. Ein Beweis vitalistischen Geschehens" (Engelmann: Leipzig, 1899, pp. 82, 3 figs.). It might be flippantly called "The Mystery of Hans Driesch."

It was begun, we are told, at San Martino de Castrozza 9 ix. 98, finished at Naples 19 xi. 98; and it was originally published in the *Archiv für Entwickelungsmechanik der Organismen*. Its importance, we read, lies in the fact that it not merely suggests but proves the necessity of recognising a new and peculiar orderliness (Gesetzlichkeit) in certain vital phenomena. It contains a proof of vitalism. And by vitalism is here meant the recognition of the unique character of organisms, the recognition of what transcends the categories of mechanism,—"diejenige Auffassung, welche in Lebensgeschenissen Vorgänge mit ihnen eigenthümlicher Elementargesetzlichkeit erblickt."

The key-note is in the word "localisation." It is especially the

"localisation" of developmental processes which appears to the author to bring out clearly the distinctive character of an organism as opposed to an inanimate system. The first illustration given may make the matter plainer.

Some four years ago Driesch showed that if a fully-formed gastrula of a sea-urchin (*Sphaerechinus granularis*) be halved equatorially, so that each half has half of the ectoderm and half of the archenteron, both portions heal up and become spherical again, and both soon show a gut divided in the normal proportions into three parts. This is a simple instance of a familiar kind of phenomenon which appears to the author to prove the necessity of vitalistic interpretation. No chemico-physical interpretation will suffice.

We cannot here summarise the author's argument, not that it is particularly difficult—for Driesch's style is limpid compared with that of many—but because of the difficulty of translating the terminology. It may be all right in German and in Germany, but we doubt if the conversion of English biologists is likely to be attained by discussions on "Der primär-regulatorische Charakter der Differenzirung harmonisch-äquipotentieller Systeme," and the like. The little book was written in about two months; it seems to us that in this, and even more in other cases, it would have been well if the author had spent an equal amount of time in making the wisdom of his counsel more generally available to busy biologists.

To return for a moment to the subject-matter. The machine theory of an organism is insufficient, since some of the most characteristic vital phenomena seem to transcend the categories of mechanism. And even if we come to understand a living creature as we understand a steam-engine, there remains the idea behind them both. Sooner or later we have to fall back upon an unknown "Gesetzlichkeit." The author's contention is that there is in the organism an elementary irreducible "Gesetzlichkeit." To overlook this, he says, is like overlooking the spider in our science of the web.

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## Morphology of the Sting in Hymenoptera.

THE embryological researches of the last twenty years seem to have securely established that the stinging apparatus in ants, bees, and wasps is derived in part from ventral segmental outgrowths, and in part from the integumentary skeleton of certain segments. In a recent paper (*Zeitschr. wiss. Zool.* lxvi. 1899, pp. 289-333, 2 pls.) Dr. Enoch Zander has analysed the apparatus in a number of representative forms, and has shown in detail how much of it is referable to (the 11th and 12th) segments of the abdominal skeleton, and how much to the genital appendages or gonapophyses. He shows further

that the latter are not developed until the larval stage is reached, and are therefore in no wise comparable to the abdominal appendages which appear and disappear during the strictly embryonic period. In fact, he confirms the conclusion of Heymons that the leg-rudiments and the gonapophysal rudiments are in their nature quite distinct.

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## Factors in the Growth of Muscle.

We have previously noticed Mr. Alexander Meek's interesting conclusion that in the post-embryonic history of striped muscles in various mammals (cat, sheep, field vole, white rat) there is a reduction in the number of fibres accompanied by a considerable hypertrophy of the survivors. Dr. B. Morpurgo got a different result in examining the white rat, and Mr. Meek briefly answered him, maintaining his position that there really is in the history of a muscle "a struggle of parts within the organism," and a resulting "survival of the fittest."

In a more recent paper (*Journal of Anatomy and Physiology*, xxxiii. 1899, pp. 596–608) he discusses the question in greater detail, and as the subject is one of much practical and theoretical interest, we quote his summing-up. "The life-history of muscle seems to be determined by (1) inherited qualities, present in the fertilised ovum, the evolution of which is controlled by (2) internal influences—internal secretion (including the effects of 'sex'), the mutual influence of the muscles upon one another, and of the fibres upon one another, and the internal variations amongst the fibres; and by (3) external circumstances—work, food, habit, and indeed, the ordinary and extraordinary conditions of extra-uterine life."

"Up to the time of birth, in at any rate the higher mammals, perhaps in all the Eutheria, hyperplasia characterises the growth of muscle; while after or about birth, hyperplasia ceases, and extra-uterine life brings about a selection of some of the fibres at the expense of their neighbours. In other words, during extra-uterine life, muscle, according to its position, suffers more or less a reduction in the number of its fibres, the degree of which is expressive of its functional importance. The surviving elements are at the same time greatly hypertrophied, and the extent to which this takes place is also expressive of the work which the muscle performs, or of which it is capable."

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## Water-Plants as Land-Winners.

In *The Naturalist* for August Mr. Albert Henry Pawson makes a brief contribution to the study of the influence of water-plants on the

land surface. "There are several ways in which these plants tend to diminish the water-space and to increase the dry land. By their own decay they form vast masses of vegetable soil in shallow waters and on water margins; by occupying running streams they moderate the flow of the current and give it time to deposit its silt; by their creeping rhizomes and spreading roots they fix the bed of a stream and prevent it being scoured and deepened by floods, and again in times of flood they serve as a sieve or strainer, arresting all floating and much suspended solid matter." This is indeed a familiar theme, but the author discusses it with freshness and with appreciation of its dramatic interest. . . . "Inch by inch, as the result of this accumulation and decay, the land creeps in upon the mere; more and more solid grows the edge; the aqueous plants retreat from the now shallow margin, the terrestrial plants advance, finding firmer footing; the sedges and reeds crowd on their floating neighbours which need space, and cannot endure the shade; these, too, press forward, and the open water grows less and less; it is invested on every side, and it is plain that its complete subjugation is now only a matter of time." It would be of interest to procure some actual measurements of the amount and rate of land-winning, and to study in minute detail the elimination which proceeds as the mere is closed up.

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## The Progress of a Great Work.

EIGHT parts are now available of "Das Tierreich"—the "Systema Naturae" up to date—which is being issued to an ungrateful world by the German Zoological Society through the medium of R. Friedländer and Son in Berlin. The *magnum opus* will give a classification and diagnosis of all living animals, and the issue of eight parts in a relatively short period permits us to hope that we shall live to see it completed. The general editor is Professor Franz Eilhard Schulze, and there are many sub-editors. Of the collaborateurs whose names are published the majority are German, but most of the European countries are represented by well-known workers. Britain is represented by Mr. W. E. Hoyle of Manchester, the Hon. L. Rothschild, Drs. Hartert and Jordan of Tring, Mr. A. D. Michael, Mr. W. R. Ogilvie Grant, and Dr. Bowdler Sharpe in London, the Rev. T. R. R. Stebbing in Tunbridge Wells, and Prof. D'Arcy W. Thompson in Dundee. The part before us is by Dr. A. Labbé, and deals with the Sporozoa; it occupies 180 pages, has 196 figures, and costs 8·80 marks to subscribers, and about a third more if purchased singly. The other parts published deal with various families of birds and mites, with a division of copepods, and with scorpions and Pedipalpi. It is not necessary to point out the magnitude of the boon which this great work will confer on systematic zoology, but perhaps it is permissible to urge individual

workers to purchase the separate parts which interest them. A subscription to the entire work is too much to expect, except from Universities, Museums, learned Societies and the like; and even some of these seem slow to recognise that the purchase is a duty. We are told, for instance, that from one of our famous university towns, with libraries, museums, and rich colleges, no single order for "Das Tierreich" has as yet been received. What an ungrateful world it is.

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## The Hopkins Seaside Laboratory.

In the *American Naturalist* for August, Professor Vernon L. Kellogg gives an account of the Hopkins Seaside Laboratory of the Leland Stanford Junior University. It is situated on the bay side of the promontory Point Pinos, which is the southern limit of the Bay of Monterey. In addition to a fauna more or less peculiar to itself, the bay contains a number of sub-tropical and sub-boreal types peculiar to the north and south zones of the Pacific coast between which it lies. "A well-known and experienced biologist of the University of Chicago, who spent a summer at the Hopkins Laboratory, has said that Monterey Bay and the Bay of Naples are much alike in the abundance and representation of species," and the laboratory has this in common with the Naples Station, that it can be used to advantage at any time in the year. The regular sessions for students are in June and July, and the fee is twenty-five dollars. Investigators prepared to carry on original work may use the laboratory and its equipment free of charge, and seventeen private rooms are placed at their disposal.

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## The Morning of Science.

It was a momentary aberration which led a great zoologist—recently lost to science—to suggest, in the enthusiasm of a retrospect, that it was now time for us to be making a list of the things we did *not* know. A very different suggestion is conveyed in a remarkable sentence in the presidential address delivered by Dr. Edward Orton at the meeting of the American Association for the Advancement of Science. After following Mr. Alfred Russel Wallace in a retrospect of the progress of science, the President pointed out that the very title of the Association indicated that the work of science was far from complete. "The founders of the Association, fifty years ago, clearly saw that they were in the early morning of a growing day. The most unexpected and marvellous progress has been made since that date, but as yet there is no occasion and no prospect of modifying the title.

We are still labouring for the advancement of science, for the discovery of new truth. The field, which is the world, was never so white unto the harvest as now, *but it is still early morning on the dial of science.*" The address was not a remarkable one, but we commend this last sentence to the attention of those who speak as if it were already late afternoon.

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## Eruption of Mauna Loa.

In the *American Journal of Science* for September some account is given of the beginning of an eruption of the volcano of Mauna Loa, on Hawaii.

Early in the morning of 4th July, one observer says, "an immense column of smoke and steam was seen rising from the crater of Mokuaweoweo. It was pierced through with the light from the fires beneath, until it glowed and shone like a column of fiery light, resplendent beyond description, and reflecting its burning glow over the whole heavens. The column seemed to be at least five miles in diameter, and rose to a tremendous height. On Tuesday the column of fire had disappeared. In place of it was the equally impressive glow of the lava as it broke from the lower side of the crater several thousand feet lower down than the column of light had been, and was thrown upward to a wonderful height by the forces which were in action. On either side of the stream, whose surface of fiery red could be seen like a line of glowing molten metal, were two cones which had formed since the eruption began. It was from these that the lava was being ejected. It was thrown up in fiery cascades high in the air. These cascades, in falling, built up the cones, and the molten lava running off from these formed the stream flowing off towards Hilo. It would be hard to say how high these cones were, perhaps somewhere between 500 and 1000 feet high, and half a mile in diameter, and five miles apart." A later account mentions three lava streams, one in the direction of Hilo, another off through Kau to the south-east, and a third towards the crater of Kilauea.

The journal from which we have cited the above also calls attention to a paper by Mr. C. J. Lyons, of Honolulu, on "Sun Spots and Hawaiian Volcanoes," published in the April number of the *Monthly Weather Review*. The author gives a table of the years of minimum sun spots for the past century, with the dates of prominent volcanic eruptions of Kilauea or Mauna Loa, showing a striking correspondence between the times of the two phenomena. As pointed out by the editor of the *Review*, however, a more thorough investigation is needed to prove that the coincidence noted is due to a real causal connection.

## The Poison of Darnel.

THAT the darnel (*Lolium temulentum*) is a poisonous grass, is an old-established and familiar fact, and experts, at least, are aware of Hofmeister's research, which disclosed the presence of two active principles: temulin, obtained as chloroplatinate, which acts upon the nervous system, and the other, determined by the oily substances and fatty acids of the seed, which attacks the alimentary canal. A new interpretation, however, has recently been suggested by Mr. P. Guérin, of the School of Pharmacy in Paris (*Botanical Gazette*, xxviii. 1899, pp. 136, 137).

He has observed in the seeds of the darnel the almost constant presence of a fungus, to which it seems to him reasonable to assign the poisonous effects. This fungus, which is not the *Endoconidium temulentum* of Prillieux and Delacroix, has also been detected by Vogl, Hanausek, and Nestler, but Guérin has shown its general occurrence, and that not only in the darnel, but in *L. arvense* With. (a variety of *L. temulentum*) and *L. linicola* Sond. as well. Its presence in perennial rye-grass is quite exceptional. Guérin has also made the suggestion that the temulin of Hofmeister may be the result of the action of the fungus upon the nitrogenous materials in the peripheral region of the seed.

The fungus, which is always present in the form of mycelial filaments, appears at an early stage in the interior of the ovary, and invades the entire nucellus. It is afterwards crowded out by the development of endosperm after fertilisation, and comes to be restricted to the region between the hyaline layer (which represents the remains of the nucellus) and the outermost endosperm. The observer found this disposition of the fungus in material from Bolivia, Brazil, Chili, Abyssinia, Persia, Syria, Spain, Portugal, Sweden, Germany, and many localities in France. In forty seeds of most diverse origin the mycelial zone was lacking only in three.

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## Coppinia.

WE are glad to note that Mr. C. C. Nutting, writing in the *Proceedings of the United States National Museum* (vol. xxi.), is able to bring forward some very definite proofs that the remarkable hydroid structure called *Coppinia* is a cluster of gonangia of *Lafoëa*. It is remarkable that Nutting's investigations made upon the species *Lafoëa dumosa* from Puget Sound were carried on independently of Levinsen's investigations on *Lafoëa fruticosa* from Greenland, in which corresponding results were obtained.

## Scientific Explanations.

THE progress of science is continually hindered by the limitations of language. What a bugbear, for instance, has been the word "law"—an innocent metaphor to the careful, but an inhibiting fallacy to the many. For, as every one knows, the "laws of nature" were for many decades the subjects of naïve personification, and made to will and act as self-sufficing governors of phenomena, while now, as Professor J. H. Poynting remarked in his opening address to Section A of the British Association, "we can only assign to them the humble rank of mere descriptions, often tentative, often erroneous, of similarities which we believe we have observed." It is indeed a fall of the mighty.

But though the word "law" has almost ceased from troubling, there remain many others which still exert their pernicious influence. Prominent among these is the word "explanation," at which we are glad to see that Professor Poynting has also made some deadly thrusts. Thickly scattered through scientific literature the student finds what are called "complete explanations," but occasionally he is confronted with the strange remark that science does not give any explanations at all. What does it mean?

The meaning is simply that while the teleological idea (of "final cause," etc.) is essential to any attempt at a complete or philosophical consideration of facts, *e.g.* to a theory of the living organism, it is irrelevant and inhibitive in scientific inquiry, which is strictly aetiological. But let Professor Poynting speak for himself.

"We have not to go very far back to find such a statement as this—that we have explained anything when we know the cause of it, or when we have found out the reason why—a statement which is only appropriate on the psychical view. Without entering into any discussion of the meaning of cause, we can at least assert that that meaning will only have true content when it is concerned with purpose and will. On the purely physical or descriptive view the idea of cause is quite out of place. In description we are solely concerned with the 'how' of things, and their 'why' we purposely leave out of account. We explain an event, not when we know 'why' it happened, but when we show 'how' it is like something else happening elsewhere, or otherwise—when, in fact, we can include it as a case described by some law already set forth. In explanation, we do not account *for* the event, but we improve our account *of* it by likening it to what we already know. . . . The aim of explanation, then, is to reduce the number of laws as far as possible, by showing that laws, at first separated, may be merged in one; to reduce the number of chapters in the book of science by showing that some are truly mere subsections of chapters already written. . . . To take an old but never-worn-out metaphor, the physicist is examining the garment of nature,

learning of how many, or rather of how few, different kinds of thread it is woven, finding how each separate thread enters into the pattern, and seeking from the pattern woven in the past to know the pattern yet to come." . . . We have heard from unfriendly critics much in regard to the dogmatism of science; it is time rather to speak of its modesty.

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## An Unsolved Problem.

In his opening address to the Chemical Section of the British Association, Dr. Horace T. Brown not unnaturally took for his subject the fixation of carbon by plants, a problem towards the solution of which he has himself made some notable contributions. The address is a fine illustration of the true scientific temper, and of the value to biologists of co-operation with workers in chemistry and physics. Definite results are still far to seek, but the address indicates a hopeful outlook, and it also impresses us anew with the danger of hard and fast statements, and with the incipient character of vegetable physiology.

The president of Section B began by pointing out that although we cease not to impress upon our students that the higher plants derive *the whole* of their carbon from atmospheric sources, the experimental evidence for this hard and fast statement is very indirect. "There can, of course, be no doubt that the primary source of the organic carbon of the soil, and of the plants growing on it, is the atmosphere; but of late years there has been such an accumulation of evidence tending to show that the higher plants are capable of being nourished by the direct application of a great variety of ready-formed organic compounds, that we are justified in demanding further proof that the stores of organic substances in the soil must necessarily be oxidised down to the lowest possible point, before their carbon is once more in a fit state to be assimilated." Along with Mr. F. Escombe, Dr. Brown has been recently experimenting in order if possible to reach some satisfactory answer to this important question. "Up to the present time," he says, "our experiments have not been carried far enough to enable us to give a positive answer to the main question, but they have already suggested a new method of attack which will enable us in the future to determine, with a fair amount of certainty, whether any particular plant, growing under perfectly natural conditions, derives any appreciable portion of its carbon from any other source than the gaseous carbon dioxide of the atmosphere."

The address contains a valuable critical account of what has been done in the past, and we venture to quote the summing-up. It does not sound altogether encouraging, but there is no object in blinking the facts. "The brilliant discoveries of recent years on the constitution and synthesis of the carbohydrates have not brought us sensibly

nearer to an explanation of the first processes of the reduction of carbon dioxide in the living plant. The hypothesis of Baeyer (that the first act of assimilation is the reduction of carbon dioxide and water to the state of formaldehyde) still occupies the position it did when it was first put forward nearly thirty years ago, although it has, it is true, received a certain amount of support from the observations of Bokorny, who found that formaldehyde can, under certain conditions, contribute to the building up of carbohydrates in the chloroplasts. . . .

"The view which Timiriazeff has put forward, that there is a mere physical transference of vibrations of the right period from the absorbing chlorophyll to the reacting carbon dioxide and water, is, I think, far too simple an explanation of the facts. Chromatic sensitisers have been shown to act by reason of their antecedent decomposition, and not by direct transference of energy, and the same probably holds good with regard to chlorophyll, which is also decomposed by the rays which it absorbs. We must probably seek for the first and simplest stages of the assimilatory process in the interaction of the reduced constituents of the chlorophyll and the elements of carbon dioxide and water, the combinations so formed being again split up in another direction by access of energy from without.

"The failure of all attempts to produce such a reaction under artificial conditions is, I think, to be accounted for by the neglect of one very important factor. We are dealing with a reaction of a highly endothermic nature, which is probably also highly *reversible*, and on this account we cannot expect any sensible accumulation of the products of change, unless we employ some means for removing them from the sphere of action as fast as they are formed.

"In the plant this removal is provided for by the living elements of the cell, by the chloroplasts, assisted doubtless by the whole of the cytoplasm. We have here, in fact, the analogue of the *chemical sensitisers* of a photographic plate, which act as halogen absorbers, and so permit a sensible accumulation of effect on the silver salts.

"When we have succeeded in finding some simple chemical means of fixing the initial products of the reduction of carbon dioxide, then, and then only, may we hopefully look forward to reproducing in the laboratory the first stages of the great synthetic process of nature, on which the continuance of all life depends."

# ORIGINAL COMMUNICATIONS.

## The Influence of the Nervous System in Organic Evolution.

By R. F. Licorish, M.D.

The majority of biologists may be at present divided into two schools, Neo-Darwinian and Neo-Lamarckian, and besides these there are others who still profess to be unable to reconcile themselves to the truths of organic evolution as interpreted by either party, and who find a prominent representative in the celebrated pathologist Virchow. In addition to the above there are a few who, like the writer, are pure Lamarckians, and who, accepting the data of Lamarck, interpret them by the light of present day knowledge, and look on "natural selection" and "survival of the fittest" as mere "figures of speech," expressive of results which have been brought about by functional and environmental adaptation. Of the two leading schools the more numerous is undoubtedly that of the Neo-Darwinians, who see in natural selection an all-sufficient cause for organic evolution. The members of the other school, that of the Neo-Lamarckians, consider natural selection as merely *one* of the factors of organic evolution, another being the inheritance of the results of the organism's post-natal experiences.

Let us look more closely at these theories to see if we cannot find therein such a relationship or analogy as would lead us to believe that a slight modification in the basis of one or the other or both will tend to more harmony than at first sight would appear to be possible. For it must be remembered that both schools are represented by able and gifted men who devote themselves to experiment and observation, and are all equally eager to arrive at truth.

Taking the Neo-Darwinians first, we find the basis of their theory to be this. Organic evolution depends on, and is carried out through, the variations which appear at the conclusion of the ontogenetic development, *i.e.* at birth. This is the basis of their theory of organic evolution. To the question, What gives rise to those variations? we have as answers:—(1) Cause unknown (Darwin); (2) Chance—a

system of "trial and error" (Huxley); (3) The reaction of the germ-plasm to external stimuli, *i.e.* the reaction of the developing organism to the external environment (Weismann).

Let us now turn to the Neo-Lamarckians. The basis of their theory is that the influences of the environment modify the organism not only during the time it is being built up, but also for an indefinite period after, assuredly during the time it is reaching its maturity or full growth, that such modifying reactions are heritable, and that on these influences the progress of evolution is chiefly dependent. As a result of limiting inheritance to the reactions of the environing influences during the pre-natal period, Neo-Darwinians have to call to their aid natural selection, whereas the Neo-Lamarckians, by extending the period of inheritance of environmental and functional reaction to maturity of the organism, can dispense to some extent with natural selection, believing as they do that the experiences of the organisms from inception of life to maturity are conserved by heredity, and that adaptation results in most cases through inheritance of those experiences. As to the strict Lamarckian, he sees no need of natural selection, believing that somatic experience is the sole cause of adaptation.

Weismann, in addition to his theory of pre-natal influence as a cause of variation, has elaborated the theory that the organism is built up and comes to maturity because the germ-plasm, during the building up of the organism, becomes distributed through it, so as to form Anlagen which are capable of developing the necessary characters and of providing for lost parts, etc. It is this feature of his work as a biologist that has made him a distinctive force in the science. And although at first he maintained that the germ-plasm as present in the germinal cells is unchangeable, more recently he has modified his position, now maintaining, as already stated, that it can react to external stimuli, and hence be changed by the influences of the environment,—an admission of the utmost importance in the re-adjustment of apparently conflicting theories. We wish to suggest an interpretation of the Lamarckian theory that may bring about a still closer approximation.

It must be acknowledged by all who make a careful study of the nervous system in its relation to evolution, and in its influence on the organism, that it is through it that all functions are carried on, and through its regulation that lost parts are renewed and injuries repaired. Moreover, it is through the nervous system, presumably as germ-plasm as well as an organised portion in the ontogeny, that all experiences acting thereon are registered and transmitted to the offspring. It has always been a surprise to me that biologists, in considering the factors of organic evolution, should have paid so little attention to the influence of the nervous system in vital processes; preferring, it would seem to me, to invest the cells themselves with

the power of reaction to the incident forces of the environment, and ignoring the desires demanding satisfaction which arise *de novo* within the brain itself. To a physician, on the other hand, the nervous system is by far the most important part of the human body. He knows that all medicines that act physiologically, and not purely chemically or mechanically on the system, do so through the nervous system. He knows experimentally that if the nerves to the organ on which a medicine acts be severed, the organ fails to respond. We know that if undue heat be applied to a portion of the surface of the body the vessels dilate and the part becomes redder, because on the heat being applied the terminal nerves telegraph to the nearest nerve-centre that help is needed to resist the irritation. Through the vaso-motor nerves controlling the calibre of the blood-vessels these dilate, probably that the increase of blood may carry off the excess of heat; the part thus making an effort to ward off injury. We may well assume that if the nervous connection were severed no dilatation of blood-vessels would take place, and in consequence the parts would suffer. Again, we know from the study of diseases that if the centre in the spinal cord for the nutrition of any special muscle be destroyed by inflammation, the muscle gradually dwindles from lack of nutrition. The whole study of pathology teaches us how, if through disease or accident defects are produced, they are remedied through the nervous influences operating correlatively on adjacent cells and tissues. Again, if a large blood-vessel be destroyed either accidentally or intentionally for purposes of cure, the small blood-vessels supplying the parts affected and anastomosing with those of adjacent parts gradually enlarge and carry on the function of the destroyed large vessel,—a fact which shows us how the distribution of blood may gradually become modified through functional change in the process of evolution of one species into another. In experiments on animals we learn that, although normally certain cells have a definite function, yet if the nerves governing those cells be severed, so that the connection between the cells and the nerve-centre is destroyed, the function of the cells ceases, and that if the centre for the nutrition of the cells be also destroyed the cells will die. Whether this is a direct result or due indirectly to the loss of nutrition has not yet been positively determined, probably it is due to the latter. Hence we can positively assert that the cells of the organism have no inherent power in themselves to exercise their function, or even to maintain their vitality, but that the nerve-centres through their connections with the cells supply that power which manifests itself as the function, and even as the vitality of the cells themselves. Thus my contention is supported, that if in the germ-cell the germ-plasm is the most important part as the bearer of the life functions, so in the finished organism the nervous system is the bearer of the like processes, commanding and controlling all life and function.

It seems to me that biologists look on the nervous system in the same light as they do other parts and organs of the system. Now, while this may be true in relation, *e.g.*, to the special senses, it must be remembered that the nervous system has also a general function, and must be looked at as belonging to and ministering to all other parts of the organism, so that the unity of all may be secured. Thus Dr. Gadow (in "The Last Link") says: "It is the physiological momentum which models the organism, and, by causing its adaptation, has produced its organs by change of function"; and again, "Each cell has a function, the more specialised the more intense it is." He attributes adaptation to the disturbance of the equilibrium of the cell, and its efforts to return to the *status quo* through increased activity. But whilst this may be true, so far as it goes, yet it is plain that Dr. Gadow ignores the influence of the nervous system, and attributes the sole power of adaptation to the cells themselves, while the foregoing remarks on the nervous system, and other facts which I shall advance farther on, go to show that the power of adaptation does not belong to the cells themselves, but to the correlative influence of the nervous system. If we restrict ourselves to the view suggested by Dr. Gadow's remarks, as in fact all Neo-Lamarckians seem to do, there is little wonder that the origin of correlative adaptations, as on the neck and other parts of the giraffe, presents a formidable difficulty, and appears almost inscrutable. The idea that life is due to some unknown and indefinable principle inherent in the cells themselves, pervades the whole of Mr. Herbert Spencer's work on "Biology," and finds its highest presentation in the writings of Virchow, so prominently brought to our notice in his recent Huxley lecture. In his chapter on the dynamic elements of life (in the "Principles of Biology"), Mr. Spencer mentions the fact that an excised liver, and in a more forcible way the excised heart, of a cold-blooded animal continues to function after detachment from the organism, but does not attribute such action to the nervous ganglia connected therewith. It must be remembered that such a continuation of function occurs, as regards the heart in particular, only in the lower organisms,[1] *i.e.* animals in which the nervous system and hence power is not so thoroughly centralised in the brain as in higher forms. In fact, there are more semi-independent ganglia dispersed through the organism. In the vegetable world we see a somewhat analogous distribution of independent centres, *e.g.* in the *Begonia*. Prof. Waller ("Text-Book of Physiology") thus writes: "Protoplasm is excitable. When any part of a lump of protoplasm is excited, the lump moves. When many lumps of protoplasm are gathered together into a homogeneous mass, excitations and movements may be transmitted from lump to lump in all directions. With higher organisation of the mass, differences of function and structure

[1] With proper precautions the excised heart of a mammal may continue beating for some time.—ED.

begin to make their appearance. Excitability, while still pervading the whole organism, becomes localised with greater intensity in some parts than in others; along some lines than along others (sense organs, nerves, and nerve-centres); in other parts contractibility becomes the salient character (muscles). To illustrate this progressive elaboration of a nervous system, we may select—(1) an amoeba; (2) a jelly-fish; (3) a frog; (4) a man." Thus we learn how gradually the nervous system is evolved, becoming, as organisation increases, more and more specialised in diversity of function, from, let us assume, invisible threads of granular protoplasm to the gray matter of the human brain, and the associated prolongations throughout the body. We must also recognise that the nervous energy is gradually diversified and intensified as evolution proceeds upward, from a mere automatic action in the protozoon, to the varied and diversified functions of man, mental as well as physical.

In his "Principles of Biology," Mr. Herbert Spencer says: "In whatever way it is formulated, or by whatever language it is obscured, this ascription of organic evolution to some natural aptitude possessed by organisms, or miraculously imposed on them, is unphilosophical. It is an assumption no more tenable than the assumption of special creation, of which, indeed, it is a modification, differing only by the fusion of separate unknown processes into a continuous process." It seems to me that, in making the above statement, Mr. Spencer wholly overlooks the power of the nervous system in rendering organisms capable of reacting to the influences of the environment. We may confidently ask, if the organism does not possess such a function, to what must we attribute the power of reaction? for unless we do recognise such a power inherent in the organism, rendering it capable of being gradually modified in relation to its needs, wants, or desires, and the incident forces of the environment, the only alternative is to believe in a power otherwise derived, *i.e.* in special creation or creations.

In addition to the evidence already adduced, I may take as an illustration of the power of the organism to respond to its needs in a definite way, Loeb's experiments to produce heteromorphosis, as cited in "The Biological Problem of To-day," by Hertwig. "In *Tubularia mesembryanthemum*, a hydroid polyp, there are stalk, root, and polyp-head. If one cut off the head, a new head will be formed in a few days, this being a case of regeneration. On the other hand, a heteromorphosis may be produced by modifying the experiment as follows:—Both root and head must be cut off from the stem; if the lopped piece of the stem be stuck in the sand of the aquarium by the end that bore the head, then the original aboral pole, in a few days, produces a head; if the lopped piece of stem be supported horizontally in the water, then each end produces a head." Hertwig goes on to give illustrations to show how, in other organisms, heads, tentacles, and

eye-spots may be induced to grow if steps be taken to initiate the changes. Here we have evidence as to how the inherent power of the organism—not the cells—may respond in a definite direction to fulfil its requirements.

In the evolution of the nervous system we must recognise two stages of development, the one gradually merging into the other. As Wilson says in his "Zoology": "In the lower or invertebrate forms of life, the nervous apparatus may be considered to be almost wholly occupied in the reception of the ordinary sensations which minister to the wants and necessities of existence, without any active or intelligent appreciation of the causes or results of the sensations thus conveyed. In the Vertebrata, on the other hand, we find the higher perfection of the correlative apparatus associated with powers which place the organism far above the rank and relations of a piece of automatic mechanism." We accordingly notice this specialisation of the correlative powers in these higher forms, evincing itself in the possession of a power of appreciation of the origin of sensations known as "intelligence"; whilst, in virtue of this latter feature, we find another and distinctive power superadded, which is devoted to the regulation of the movements of the body, and which is known as the power of "volition" or "will." Now, it is the possession of these varied and distinctive features, due in the lower orders to reflex nerve action, and in the higher to a species of intelligence, that led Lamarck to denote as needs, wants, or desires the processes through which animals satisfy the physical wants of their bodies. For whilst, in the lower, that process by which the exigencies of the organism are satisfied, may be considered as in response to a need, as in the vegetable world; in the higher, a species of will is manifested, as the will for food, etc., and this may certainly be construed as a desire.

Professor Conklin, in an able article on the factors of organic evolution (in "Footnotes of Evolution," by Professor Jordan), has arrived at some inconclusive deductions, under the head "Use and Disuse." He remarks: "I take an example which will serve as an illustration of a whole class. Jackson says that the elongated siphon of *Mya*, the long-necked clam, is due to the habit of burrowing in the mud; or, to quote his own words, 'It seems very evident that the long siphon of this genus was brought about by the effort to reach the surface induced by the habit of deep burial.' It certainly would be pertinent to inquire (asks Professor Conklin) where it got this habit, and how it happened to be transmitted. It is surely as difficult to explain the acquisition and inheritance of habits, the basis of which we do not know, as it is to explain the acquisition and inheritance of structure which are tangible and visible." That Professor Conklin does not understand the acquisition of habits shows clearly that he does not understand Lamarck. I have already in these pages explained my interpretation of the nature of functions, but my point may

be further illustrated in this way:—The functions of organisms, especially the higher ones, are divided into two sets—the first "*vital*," the second "*organic*." The "vital functions" are those of nutrition, reproduction and protection, those on which the life of the individual and the perpetuation of the species depend, and which in the higher organisms are satisfied through desire. On the other hand, the "organic functions" depend on the structure of the special organs. Thus, for instance, in respiration, the exchange of gases is effected according to the special structure of the breathing organs. Now all habits of animals are acquired through the vital functions originating, either as a reflex action as in the lower, or in response to desire as in the higher orders. Singularly enough Mr. Herbert Spencer, instead of recognising the importance of the acquisition of habits, has discussed the matter under the head "distribution." Now, whilst the distribution of animals does lead to the acquisition of new habits, it is only an indirect cause, the direct cause being the efforts made by the animals themselves to suit their life to the new circumstances. And they do this to satisfy their vital functions—in particular, that of nutrition. There are woodpeckers in the United States that feed on fruit, and Darwin saw woodpeckers in Patagonia feeding on insects in the air. How was the new habit of feeding on other than their customary food acquired? Clearly in their desire to satisfy the craving for food. Darwin also saw and examined certain birds, originally webbed, showing the beginning of web-disappearance. But, and here we see the significance of such a fact, the birds in which he saw such a beginning of web-disappearance had become habituated to another mode of life than that on water. They had then acquired a new habit of life, and through disuse the web had begun to disappear. We thus learn that the habits of animals, whether through reflex actions as in the lower organisms and as in plants, or in the higher orders in response to the desire to satisfy the vital functions on which the life and perpetuation of the species depend, are the results of the demands which the exigencies of the organism require for the satisfaction of the vital functions. These demands of the organism Lamarck clearly understood; and why, in the present day, biologists fail to consider them is a matter of surprise to me. Not to recognise them in the light in which Lamarck, and doubtless Goethe too, recognised them, renders the doctrine of organic evolution less intelligible, and thus more difficult to harmonise with other truths.

Let us now consider the question: Why, and how, are the modifications functionally produced by change of habits inherited? As already stated, I distinguish between the Neo-Lamarckians and the true Lamarckians in this way. The former believe in the inheritance of functional modifications, but only as brought about through cell-activity, thus failing to see how correlative parts are modified; whereas the secret of the true Lamarckian's position is, that he understands

both how separate parts as well as correlative parts are modified. Present-day knowledge goes to show that such changes are brought about through the co-operative influence of the correlative brain centres. Yet, strange it is that the leader of the Neo-Lamarckians, Mr. Herbert Spencer, while he cannot see how natural selection can produce such changes as are shown in the neck, etc., of the elk, has to fall back on natural selection to explain the modifications shown in the fore-quarters of the giraffe, a more difficult matter than the elk's neck to bring under the influence of natural selection. If the changes in the elk's neck cannot be explained by natural selection, how can the parts of the giraffe, a more marked form of correlative function change, be so explained? If natural selection is to be ruled out as regards the elk's neck, it must more surely be ruled out as regards the giraffe.

I have already stated that Weismann, like a true Lamarckian, attributes variations to the influences of the environment on the germ-plasm during the ontogenetic development of the body. That being granted, we can readily perceive how change of habit can produce in time change of characters through inheritance of the functional modifications brought about through the change of habit. It is well known that many animals have, not one source of food supply, but several. A bird that visits a flower for honey may also be insectivorous. One source of food supply failing, the habit of constantly satisfying hunger from another is taken on; and this, by change in the method of feeding, leads to the increase of use of certain characters which co-operatively are brought into action, and the disuse of certain other characters. In this way distribution of animals or change of conditions *in situ* leads to new habits. But does the new habit modify the species in the direction of better adaptation to the new mode of life? I would answer that if the experiences of the mother influence the foetus, and act as external stimuli on the germinal cells, as is allowed by Weismann, we must see that changes in that experience, as brought about by a new habit, must be reflected on the foetus, producing a variation in the direction of better adaptation. And this process of better adaptation in each successive offspring must, in time, render the species fully adapted to its new mode of life. We find here not only the cause of variation, but the gradual process by which species through a change of habit becomes adapted to their new life. As the functional changes affect characters, new species are produced. Now, assuming that the Neo-Darwinians admit this *modus operandi* of the formation of new habits, our explanation of the inheritance of functional modifications of characters would harmonise the two schools, *i.e.* if we allow that Weismann represents the Neo-Darwinians.

Let us now consider how functional changes, as brought about by a change of habit, modify anatomically the characters affected. It must be plain that all modifications of form must have been

wrought by change in the environment, as otherwise heredity could never have any characters to work on. If that is not allowed, we must fall back on blind chance, or on the insinuation of some unknown power. Changes in environment can only be partial, since a complete change would destroy all organic life. But where changes are partial, and extending over vast periods of time, great changes may occur in the organism, as in the evolution of whales and seals from land animals. Now, where there are changes in environment leading to new habits in order to satisfy the vital functions, the organs or characters affected by the change of habit, being used in excess of their former use, are further developed, *i.e.* their cellular elements are increased, either absolutely or relatively or both, since increase of use means increase of nutrition. But the cell activity is brought about, not directly, but indirectly, through the connection with the nerve-centres. Hence the increase of exercise in the nerve-centre leads to increase of nutrition, and this in turn to increased development of the nerve-centre. Thus, with the increase of function, there is also increase in size of the characters affected, and of the brain centres presiding over them. Increased use of a muscle leads to increase in size, and the brain centre of the muscles must also be changed in some way, for it too has done increased work. We know that the memory may be strengthened by exercise, and so with other special mental faculties. So too, as regards the special senses, the sailor's eyesight is always better than the landsman's.

It is important, however, to remember that such changes take place chiefly in the young, and hence the importance of our contention that the condition of the maternal body—cells, tissues, and organs—affects the vitality of the developing ovum. The maternal conditions, acting as external stimuli to the ovum, must, as Weismann admits, affect the foetus, and I argue that they will produce such modifications as will bring the latter into harmony qualitatively and quantitatively with the maternal body. And as the general environment reacts on the mother, and the mother on the embryo, it must be evident that the general environment has some influence on the developing germ or embryo. Now as the general environment of a mother in her successive production of offspring must vary, so too must the offspring vary.

Assuming that the nervous system is to the fully-formed organism what the germ-plasm is to the ovum, we must see that there must be the same difference between the cells of the nervous system and the cells of the other portion of the organism as between germ-cells and somatic cells, for whereas the nervous system represents the whole body, a *multum in parvo*, and can induce the production of all kinds of cells, the somatic cells can only reproduce through the nervous system *cells of their own kind*. The egg-cell contains, as Naegeli says, all active specific characters as truly as the adult organism. What I

maintain is that each specific centre for a character in the germ-cell is represented in the nervous system by a specific centre for controlling such a character, *i.e.* that the specific centre in the germ-cell has developed into a specific nerve-centre in the central nervous system. It is only in this way that we can recognise the unity of the organism, and can understand the specific morphological characters of the organism.

In conclusion, I would call attention to a passage in Prof. Jordan's work, "Footnotes of Evolution," which expresses the position of Lamarckians as well as Neo-Darwinians. "The fitness by which organisms have been perpetuated is simply obedience or adaptation. Those which survive are fitted to the conditions of life. In other words, they are obedient to those conditions. Hence we may define the process as one of the survival of the obedient." Now whilst, as I have said, the above expresses well the conclusion of the Lamarckians as well as the Darwinians, the different standpoints of the two schools must not be overlooked. The Darwinian believes that obedience is at first restricted to the few in which favourable variations occur, and gradually through the production of more and more of such variations to the many; whilst the Lamarckian, recognising that the power to be obedient is a general law of nature, sees the obedient as the many, the disobedient being the few abnormal ones. Hence the main difference between the two schools resolves itself into this: The Lamarckian sees a general law of obedience, the Darwinians a law of opposition leading to a forced obedience.

If obedience is through natural selection operating on all characters, it is almost impossible to conceive that favourable variations as regards all characters can be present at the same time and in the same individual; if such should not be the case it must lead to the perpetuation of unfavourable variations as regards the unfavourable characters. Again, that the most favourable variations are weeded out through sexual intermingling is proved by this fact which is taking place constantly in all tropical countries. If the product of a black and a white person—a mulatto—with the favourable feature or character—the brown colour—intermarries with a white, and the descendants do the like, the favourable character—the brown colour—gradually disappears, until the descendants are indistinguishable from Europeans. Here the favourable character, which ought to have been preserved through natural selection, is gradually weeded out.

BARBADOS, W. INDIES,
*July* 1899.

# The Fauna of the Sound.

Abstracted by F. A. Bather from the Swedish of Dr. Einar Lönnberg.

In two papers, entitled "Undersökningar nörande Öresunds djurlif," and "Fortsatta undersökningar," etc., and issued as *Meddelanden från Kongl-Landtbruksstyrelsen*, Nos. 43 and 49 (Upsala, 1898 and 1899), Dr. Einar Lönnberg has published the results of some researches made by him during June 1896, July 1897, and August and September 1898, under the auspices of the Swedish Office of Agriculture (Landtbruksstyrelsen). The language in which these papers are written, as well as the place of their publication, must prevent the majority of English readers from appreciating their considerable interest. The following attempt to present Dr. Lönnberg's general conclusions may therefore have some value.

Öresund is the narrow tract of water that divides Scania, the southern province of Sweden, from Själland, the island on which Copenhagen stands. Travellers from Denmark to Sweden cross its southern end as they go by steamer from Copenhagen to Malmö, while its northern opening is seen by the visitor to Elsinore. The Sound, as we usually call it, forms one of the connections between two sharply separated provinces of marine life—the brackish Baltic and the salt Kattegat. From the biological point of view it must be restricted within rather narrower limits than those usually assigned to it. Dr. Lönnberg draws the northern boundary from Hellebaek, a little north of Elsinore, to the projecting reef of Hittarp on the opposite Swedish coast. The southern boundary is marked by a broad bank stretching across by the islands of Saltholm and Amager, just south of Malmö and Copenhagen.

It is of course the case that the Sound, no less than the neighbouring seas, has been the subject of investigation by many naturalists. The Germans, for example, have their "Kommission zur wissenschaftlichen Untersuchung der Deutschen Meere in Kiel," together with the "Biologische Anstalt auf Helgoland"; Denmark has published "Det videnskablige Udbytte af Kanonbaaden 'Hauchs' Togter i de Danske Have inden for Skagen," and the reports of Dr. C. G. J. Petersen from "Den Danske biologiske Station"; while the Norwegian "Nordhavs-

expedition," and the writings of many other Scandinavian naturalists, trench more or less upon the region herein considered. Dr. Lönnberg also admits that his time and means have both been limited. He had only a little sailing-boat, with dredge and trawl no bigger than could be worked by hand. These facts add to the suggestiveness of his results. For, if he has been able, with such feeble opportunity, to add to the list, not merely of the Swedish marine fauna, but of forms new to science; if his work already enables him to foreshadow conclusions of scientific no less than practical interest, then it is clear that there is room for continued and still more detailed investigation. Considering the fluctuations in the number and kinds of fish that are said to have taken place in the Sound during this century, the mere list of captures has a certain value for comparison with past and future lists. Indeed the only previous list is that which Oersted published so long ago as 1844, in his little book "De regionibus marinis."

In a short introduction Dr. Lönnberg discusses the conditions governing the distribution of life in such a region as Öresund. The changes of wind and of current, which so frequently take place, may in a day or two completely alter the composition of the minute surface fauna, and thus induce a corresponding migration of such pelagic fish as herring and mackerel, which feed on these idly drifting organisms. To be of practical value, the study of such changes must continue from day to day. It is otherwise with the sedentary or slowly moving life of the bottom, and with the fish that feed thereon, such as cod and flat-fish. The constituents of this fauna, abiding in the same place from year to year, must be suited to the conditions there obtaining, and must be able to survive all those changes in salinity, temperature, and the like that may occur in the various seasons. Slow geological changes may have caused the fauna to alter slightly from its original composition, and may have eliminated some of its earlier elements; but their effect is more likely to be seen in a less favourable development of individuals. Experiment and observation have shown that many marine species can accommodate themselves to a slow reduction of salinity, or other change in the chemical composition of the water, although they may show signs of the change in their smaller size or less calcified skeletons. A difference of depth is not so important, and in any case since the so-called *Littorina*-age, which in the Baltic area was the immediate forerunner of present conditions, the amount of shallowing has not exceeded 5 metres. This, on the data generally accepted, and assuming a regularity in the change, implies a lessening in depth not more than 5 centimetres a century.

It follows from the arguments here briefly outlined that past fluctuations in, and the present distribution of, what one may call the edible fauna, with all their practical effect on the human neighbours, may be best interpreted by a detailed study of the present fauna,

and of the nature of the bottom, in which latter the varying character of the flora must be included.

Here we cannot reprint the annotated faunal list given by Dr. Lönnberg; nor is this needed, since the universal language of systematic zoology will enable any one specially interested to learn from the papers themselves what species have been found.

The list of fish is complete, being supplemented from other sources than Dr. Lönnberg's own captures. It includes 98 species, of which 92 are purely marine. Of these latter, 41 are southern forms, stretching down to the Mediterranean, and never passing above the Arctic circle; 30 are northern forms, stretching from the Arctic seas no farther south than the English Channel; 11 have an intermediate or West-European distribution; while 10 have a wider and less determined range. But when we consider the distribution of those fish that are permanent inhabitants of the Sound, or that appear there regularly year after year, the proportions are reversed. Such species number 47, and of them 14 are southern, 22 northern, 6 intermediate, and 5 wide-ranging. Comparison with neighbouring areas brings out several points of interest, of which a few may here be noted. The fish-fauna of Helgoland is less numerous (78 species), but as a whole the proportions of northern, southern, and intermediate forms are about the same as for Öresund. Among permanent inhabitants, however, Helgoland reckons a larger percentage of southern forms. The west Baltic has a fish-fauna of about 95 species. Many of these are fresh-water forms, of which only a few occur in the Sound. Of the salt-water forms almost all occur in the Sound, which also contains 21 species not found in the west Baltic. These latter, however, are more or less occasional visitors, and of them 10 are southern, 3 northern, 6 intermediate, and 2 wide-ranging.

In this fish-fauna the oldest stock consists of the northern species, which could live in these regions during or soon after the glacial period. Then, too, their range extended farther to the south, so that most of them reached the coast of France, and some got even as far as Spain. For others, however, such as *Drepanopsetta platessoides*, the southern limit was already reached in the Sound. A final class consists of pure relict forms, such as *Lumpenus lampetriformis* and *Cyclogaster liparis*. As the climate improved, species of southern origin could by degrees settle in the Sound.

The occasional visitors in the fish-fauna follow the various kinds of water in the marine currents; thus the southern species come with the warmer and salter water in summer and autumn. When the conditions are altered by an influx of some other water, also when the temperature is lowered, many of these fish sicken and are thrown up on the beach, so that just before winter many southern fishes are found in this way. The southern immigrants are observed from June to December. The northern species that come with currents from the

north, are usually found from February till April. For all these fish the Sound forms, as it were, a large net with deep and wide intake towards the north, narrowing funnel-wise between Helsingborg and Elsinore, but widening again and deepening by Hven Island and Landskrona; but for a large part of the migrants the passage is completely closed by the sill-like shoal between Malmö and Saltholm.

Passing to the lower forms of animal life, Dr. Lönnberg mentions only such as he has himself observed, and gives careful notes on their habitats. The northerly nature of the fauna, already exemplified by the fish, is far more marked among these less wandering groups.

*The Oscidians "have a distinctly Arctic stamp."*

Among Mollusca, the bivalve fauna is almost entirely northern. Of 32 species, 14 are purely northern, while all the rest have been recorded from Arctic Norway. Of the 22 prosobranch gastropods, 9 are northern, 8 wide-ranging but chiefly northern, 2 wide-ranging but chiefly southern, although they are found at Lofoden as well as among glacial fossils; 3 alone are purely southern forms. Of the 4 shell-bearing opisthobranch gastropods, 1 is purely southern, but the 3 others, though having a southerly distribution, are found in Arctic regions. The 3 nudibranchs are all northern. Three of the chitons are purely northern; the 2 others wide-ranging, but do not reach farther north than Lofoden. In short, of all the 68 molluscan species, 42·66 per cent are purely northern; the same proportion stretches from the Mediterranean to the Arctic seas; 8·82 per cent find their northern limit at Lofoden; only 5·88 per cent are purely southern. A comparison of the measurements of 55 shell-bearing species from Öresund, the Kattegat, Arctic Norway, Kiel Bay, and the Mediterranean, gives the following results. The molluscs of the Sound are, as a rule, smaller than those of the Kattegat; those that are larger or of equal size are all Arctic forms. Compared with the molluscs of Arctic Norway, those of the Sound are smaller no more often than they are the larger, or of equal size. The molluscs of the Mediterranean are usually larger than those of the Sound, but the contrary is sometimes the case. The molluscs of the Kattegat are generally larger than those of Arctic Norway. Species that are common to the Kattegat and the Mediterranean are twice as often the larger in the Kattegat. This shows that salinity alone is not the effective factor in this case, but that other causes co-operate.

Among 16 species of the higher Crustacea, 7 are northern; 4 wide-ranging and reaching the Arctic; 3 are west European; and 2 purely southern.

Of the 41 or 44 species of Chaetopoda found by Dr. Lönnberg within the Sound as restricted by him, no less than 25 are purely northern; 12 are wide-ranging, but at least two-thirds of these have

been found off Greenland; only 2 or 3 are southern, and 2 intermediate. The northern character is even more manifest when one includes all species recorded in literature as found in the Sound, many of them, however, at its northern boundary. Fully half of the 48 are northern, and only one purely southern. The Chaetopod faunas of the Skagerack and Kattegat, on the other hand, contain more southern elements, and especially a large number of species with west European distribution—neither Arctic nor southern—a group that is but sparingly represented in the Sound.

The Bryozoa have not yet been thoroughly worked out; but of the 9 species found, as well as those previously recorded, *Membranipora membranacea* is the only purely southern form; the rest are either northern or wide-ranging, but for the most part found in Arctic seas.

The Echinoderma, of which there are 19 species, have a distinctly northern character. The 3 holothurians are northern. Five starfish are northern; the sixth, *Asterias hispida*, now first found in Swedish waters, is a Shetland form. Of the sea-urchins, 1 is northern and 2 wide-ranging. The brittle-stars comprise 3 northern forms, 2 wide-ranging, but tending more to the south, and 2 (alone among the Echinoderms) purely southern. There are in the Kattegat 18 more species of Echinoderma than in the Sound, and it is most suggestive that of these 8 are southern, 7 intermediate, 1 wide-ranging, and only 2 northern. Obviously the Echinoderm-fauna of the Kattegat is far more southern in its composition than is that of the Sound. So, too, among the 29 species of Echinoderma, known from Helgoland, only 9 are northern, the rest being wide-ranging or southern forms.

Only 16 species of Hydroidea have as yet been determined, but these add *Acaulis primarius* and *Cuspidella grandis* to the list of the Swedish fauna, while *Lovenella producta* and *Opercularella lacerata* have not before been found in Öresund. This part of the fauna has a northern character, more pronounced than that of Bohuslän, for example, from which, though it lies farther north, many of the northern species are absent.

The list contains notes on other zoological groups, but nothing of sufficient importance to be mentioned in this short abstract. The foregoing account is based on Dr. Lönnberg's first and larger paper; the second paper adds only three or four species, among which may be mentioned the new Hydroid, *Clava glomerata* (see *Zoologischer Anzeiger*, No. 578).

As already observed, the chief factor in the distribution of species within the Sound itself is the nature of the bottom. Dr. Lönnberg distinguishes the following regions and sub-regions: Shore-regions; Zostera-region; Alga-region, with Laminarian, Furcellarian, and Coralline sub-regions; deep-water, with bottom either of dead zostera, or mixed, or sand, or clay. Of course each of these divisions merges into those adjoining, but on the whole they may be characterised thus:—

1. Shore-region, reaches to a depth of 2 or 3 metres, with sandy bottom and a vegetation of Ulvaceae, *Fucus*, *Chorda*, a number of fine, thread-like green algae, *Potamogeton pectinatus*, and some zostera. For the list of characteristic species, reference must be made to the original paper. Most of them pass far up into the Baltic.

2. Zostera-region, usually with sandy bottom and zostera, from 3 to 15 metres deep.

3. Alga-region, broadly speaking from 15 to 20 metres deep, divided into (*a*) Laminaria sub-region, usually with a soft bottom of mud, often mixed with stones and shells; (*b*) Furcellaria sub-region, forming thick carpets with admixture of various red algae; (*c*) Coralline sub-region, with calcareous and red algae on a stone bottom. This last is more distinct in the nature of the bottom, and has a fauna more peculiar to itself, including many chitons.

4. Deeper water, without vegetation, usually outside the 20 metre line, subdivided thus: (*a*) dead zostera bottom, clayey or muddy with many dead leaves of zostera, which give it firmness and serve as food for several animals. This usually comes next to the Alga-region, and may reach a depth of 33 metres. When the zostera leaves are fewer and the clay mixed with sand, it passes over into (*b*) mixed bottom, which often contains many shells in a floor of sand and clay mingled in varying proportions; thus it passes into the two following: (*c*) sand bottom, often with shells, shell gravel, or shell sand; this is found in places where the current is strong enough to sweep away the finer mud, which goes to form the chief part of (*d*) clay or clay-mud; this, which is found in the greatest depths, is loose or oozy, but has no evil odour of decomposing organic substances.

In the Sound these various kinds of bottom do not, as in more open seas, succeed one another from shallower to deeper water, but depend rather on the currents, so that sand or mixed bottom may be found at greater depths than clay or mud.

From the facts given at length in the original papers it appears that almost every species of animal shows a preference for one particular kind of bottom. In many cases this is because they are suited to a certain mode of life, so that if, after the breeding period, the larvae sink on to a spot with unsuitable bottom or where other conditions of life obtain, the animals die off at once or in a short time. For instance, if mud from the depths be passed through a fine sieve, dead shells of young *Astarte* are often found, sometimes in great numbers. This shows that *Astarte* cannot exist on the soft mud. Its shell is too heavy: it sinks and perishes. On the other hand, its thick shell with stout epidermis is fitted to withstand rubbing and knocking against sand and pebbles, and a bottom of such nature is firm enough to prevent the shell from sinking into it. *Leda*, on the contrary, with its shell swollen up in front and beak-shaped behind, with its strong foot spread out like a sheet, is well equipped for living and boring in the

clay: so too is *Abra* with its thin light shell and long siphon. *Cyprina* also is prevented by its almost ball-shaped shell from sinking in the clay; at the same time it prefers a bottom mixed with sand. The long arms of the sandstar, *Amphiura*, and the felted spines on the under side of the heart-urchin, *Echinocardium*, must also bear up the animal's body on a loose bottom. *Natica*, which burrows with its outspread foot, has not much to fear from clay, though it usually prefers some other kind of floor. *Buccinum* thrives in clay: it is strong enough to work itself along there. *Cardium fasciatum* is found on all sorts of bottom. But the animals that do best in the clay mud are a number of Chaetopods. When, however, the clay is made firmer by admixture of sand, or by a carpet of dead zostera leaves, a far richer fauna is able to develop.

Difference of depth has here scarcely any effect on the distribution of species, since the whole Sound is so shallow that it would come within the littoral zone as usually understood. Such difference as there is has an indirect influence through its effect on the water. The southern sill and the narrowing between Saltholm and Scania cause the brackish currents from the Baltic to reach right to the bottom; but as the Sound widens again these currents broaden and thin out, so that their effects do not stretch so deep. Thus the bank between Malmö and Saltholm forms a complete barrier against the marine forms; the southern end of the Sound is occupied by a brackish water fauna, and the limit between this and the deeper salt water fauna gradually rises nearer the surface as it approaches the northern end of the Sound. The southerly increase of conditions unfavourable to a purely marine fauna differentiates the whole fauna into four classes according to the distance to which each penetrates the Sound.

We are now in a position to discuss the origin of the fauna of the Sound. We have seen how, in class after class, the species of purely Arctic or partly Arctic distribution outnumber those with a west European or more southern range. We have noted also that the proportion of northern forms is greater in the Sound than in neighbouring seas. Further than this, there are in the Sound a number of northern species which are not found in the Kattegat at all, or only in its most southerly portions, or which, if they do occur over the whole Kattegat, are not found in any quantity till one comes south. The Echinoderms furnish specially good examples. The holothurian, *Phyllophorus pellucidus*, is fairly common in the Sound, but only one specimen has ever been taken in the Kattegat, and that was in its southerly extension. It is not known off the more northerly Bohustän. But this species is typically Arctic; it occurs in the Norwegian Finmark and at Spitzbergen, and specimens found there cannot be distinguished from those dredged in the Sound. On the other hand, the *Phyllophorus* that occurs off western Norway, as well as the allied English form, both differ from that of the Sound. *Phyllophorus drummondi*, also taken in

the Sound, is another Arctic form that, on the coast of Norway, increases in number towards the north; it has been found in the Kattegat only at Samsö. Another holothurian, *Psolus phantapus*, does, it is true, occur in various parts of the Kattegat, but is more usual in its south-west corner, and is common in the mid-region of the Sound. The same is the case with *Cribrella* and *Solaster endeca*, although these star-fishes are not quite so common in the Sound. *Asterias muelleri* has only been observed a few times in the Kattegat, and then in its southern portions; but it is not rare in the Sound. *Crossaster* also increases in number towards the south. Again, a common brittle-star of the Sound, *Ophiopholis aculeata*, is rare in the Kattegat until its south-west portions are reached. In fact, as shown by C. G. J. Petersen, all the Arctic Echinoderms of the Kattegat are concentrated towards the south-west. Many similar examples are seen among the Mollusca, e.g. *Modiolaria nigra*, *Modiola* and *Bela trevelyana*. *Astarte borealis* is exceedingly rare in the Kattegat proper, and is also rare in southern Norway, but is common towards the Belt and in the Sound. *Chiton albus* is found only in the southern Kattegat, the Belt, and the Sound, *C. marmorens* begins to be common below Samsö, and so on. It would take too long to go through all the other classes of animals; one can just allude to such purely Arctic forms as *Lithodes* and *Mysis oculata*, which are found in the Sound, but not at all, or very rarely, in the intervening seas. It is clear enough that a large number of Arctic forms occur in the Sound (as also in the Belt) far removed from their natural area.

How is the existence and origin of this Arctic element to be explained? There are two possibilities. Either it has wandered in recently and is constantly recruited, or it has persisted here from a by-gone age when conditions differed from those of to-day and were of an Arctic nature, like those which the forms in question now find in their proper home.

The first hypothesis seems at first to be supported by the existence of marine currents which every year, about February and March, bring water from Greenland to the Skagerack and the Kattegat. The fauna of the Sound and the southern Kattegat might therefore be recruited by larval forms floated across from Arctic regions in these currents. But to this view there are various objections. It is not likely that a larval form should float in the water long enough to complete the journey from Greenland to the southern Kattegat, since this occupies about half a year. The time required by the various forms to pass through their pelagic larval stages is not known for every case, but it can hardly be so long as half a year. Théel, for example, has shown that *Echinocyamus* needs no more than two months to develop from the egg into a sea-urchin crawling on the bottom. Mortensen has observed that masses of larvae of *Asterias rubens* and *Ophioglypha texturata*, floating in the Limfjord, remained there only a few days. The same author remarks that the floating larvae of Echinoderms are found chiefly near

the coasts, and do not belong to the true plankton of the high seas. The Hensen Plankton-expedition only once got as many as three Echinoderm-larvae at any distance from land. Only five species of Echinoderm-larvae were found out in the Atlantic, and three of these were in the Sargasso Sea.[1] Again, this first hypothesis does not explain why it is that these Arctic forms should be found in Öresund and not in other places, such as the northern Kattegat, where the opportunities for their development seem equally favourable. Moreover, many of the forms in question stretch north-eastwards along Finmark to Spitzbergen and the Kara Sea, but are not known from the coasts of Greenland: such are *Phyllophorus pellucidus*, *P. drummondi*, and *Asterias muelleri*. Some species of the Mollusca too are absent from Greenland, *e.g. Bela trevelyana*. But from Spitzbergen and the surrounding seas no current leads to the Kattegat. Then, too, if the first hypothesis were true, we should expect to find many other Greenland species, which, as it happens, are absent not only from the Sound but also from the Kattegat and Skagerack. Of twenty-nine species of Echinoderms found in Greenland, only eight occur in the Kattegat and the Sound. If some can cross, why not others? Take the case of *Cucumaria frondosa*, a holothurian common in Greenland waters, and with so wide a distribution that it stretches down America as far as Massachusetts, and down Europe from the North Cape to the English Channel. Yet it is absent from Bohustän, the Kattegat, the Sound, and Helgoland. This is a strong argument against the Greenland current theory. A still more forcible objection is furnished by the fact that some of the starfish in question (*Cribrella*, *Asterias muelleri*, *Crossaster papposus*), and perhaps other of the Echinoderms, have no pelagic larval stage at all.

It is clear that the first hypothesis fails us at many points. We have then to consider the second, and to inquire how long and wherefore these forms have remained in a district so isolated from the rest of their area of distribution. Two main groups of conditions determine the persistence of an animal in a given locality. One group includes the external chemical and physical conditions; the other, the relations of the organic world. The Sound, therefore, must afford conditions suited to the existence of Arctic animals, and at the same time less suited to the more southern forms with which they have to struggle. Arctic forms are accustomed to a low temperature, and also to great changes in the salinity of the water consequent on the melting of the ice.

[1] Two considerations seem to be overlooked by Dr. Lönnberg. First, the fact that a species *can* develop rapidly does not prove that it *must*. Experiment has shown that development may be greatly retarded by varying the conditions, and, for all we know, the necessary stimulus to complete development may be wanting in the current from Greenland so long as it is far from land. Secondly, as Alexander Agassiz, for one, has insisted, Echinoderms can be transported in other than the larval state; the young sea-urchin itself can be floated along. Especially is this the case when drift-wood or floating sea-weed comes to their aid.

Southern forms, on the contrary, enjoy a comparatively equable and high temperature and constantly salter water. Now, the water of the Sound is at all times of comparatively low salinity, and is, under the influence of winds and currents, liable to still greater reduction. Moreover, its shallowness, the influx of cold Baltic water, and the cold winds blowing from Sweden, combine to lower the temperature in winter almost to freezing-point to great depths, if not to the very bottom of the whole Sound. These conditions thus, while suited to the hardy northern species, are distinctly unfavourable to the more southern forms with which they contest the ground.

It is then intelligible that Arctic forms should continue to live in the Sound; but, since they have not entered recently and are not now coming in, they must have persisted there or thereabouts since a time when Arctic conditions were so widely extended that they embraced the now isolated Sound as well as the intervening areas. That took place during late glacial times. During the changes that succeeded, these Arctic forms must have changed their home and given way before the fresh-water streams from the *Ancylus*-sea;[1] but though many doubtless perished, a number of forms could brave it out, thanks to their power of resisting brackish water. When a fresh sinking of the bottom of the Sound let the salt water burst afresh into the Baltic, the Arctic forms came along with it by degrees, into the Sound and the Belt, and perhaps yet further; in this way they withdrew from the contest with the more southern forms that were now thronging up out in the Kattegat. This struggle with the more southern and more typically marine forms was then for a time even harder than now, since for a long period the water was much salter than at present, so that the oyster, *Tapes*, and other forms now extinct in those parts, could thrive there. It is therefore probable that it was just at that time—the *Littorina* period—that the break took place in the connection between the northern and principal area of distribution of the Arctic forms, and the more southern isolated districts, such as the Sound, where those forms still exist. After a time the Kattegat again became less salt, and a part of the southern marine forms (*Ostrea*, *Tapes*, etc.) died out. Thus began the existing state of things, in which the Arctic forms again found favourable conditions of existence, and possibly again extended their range.

Thus it is that, in the existence of an Arctic element, the fauna of the Sound presents a phenomenon like to that of Gullmarsfjord in Bohuslän, and many Norwegian fjords, in which Arctic animals are found far south of their proper limit. Such persistent types are called relicts; and thus the fauna of the Sound may to a certain extent be called a relict fauna. The same term is perhaps also applicable to the fauna of the Belt. The conditions in these sounds are in a way like those in a fjord. In both cases is a narrow, enclosed water which com-

[1] Occupying more or less the district of the present Baltic.

municates with the sea at one end, and which is subject to a varying influx, in the one case, of fresh water, in the other of brackish water; in either case with the same result. Öresund especially is like a fjord, since the bank between Malmö and Saltholm forms a sill which prevents the deeper and salter layers of water from flowing right through into the Baltic. Those are the conditions that in great measure explain the composition of the fauna of the Sound.

The first paper ends with "some words on the vegetation of Öresund." Although the details are not full enough for any argument to be based on them, it is noteworthy that of the forms mentioned only two are lacking within the polar circle. At all events the facts corroborate the views above expressed regarding the origin of the fauna of the Sound.

The second paper presents a more detailed study of the extreme southern portion of Öresund. It contains many facts of scientific and practical interest, especially concerning the herring. But here we can only note that the general statements and explanations of the former paper are fully confirmed.

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# Suggestions upon the Origin of the Australian Flora.

*Continued from page* 212.

By Spencer Moore, B.Sc., F.L.S.

Our scanty knowledge of the geology of the West Australian desert has recently been materially added to by Mr. Victor Streich,[1] who traversed the southern part of the desert lying between Mount Squires on the eastern border and Yilgarn on the west. Mr. Streich finds that Mesozoic rocks, covered in many places by abundant tertiary deposits, extend from Mount Squires as far west as Queen Victoria Springs, except in one place where Palaeozoic cliffs were seen. The rocks regarded as Mesozoic are clay, jasper-rock, conglomerates, and quartzite sandstone, and they are assigned to this age on lithological grounds alone, there being no fossils in them, but their lithological and stratigraphical features being the same as in the typical area outside the western colony. West of Queen Victoria Springs there are quartzite ridges, and at the Fraser Range hornblendic schists are met with. From the Fraser Range towards Lake Lefroy and the Hampton Plain, that is in the Coolgardie district, a series of metamorphic rocks are met with, the country having a general elevation of 1200 to 1500 feet above sea-level, while to the west lies an immense high plateau, 1300 to 1400 feet above the sea, terminating at the steep western escarpment of the Darling Range; there are several formations in this plateau, the granitic and the flanking schistose being the most conspicuous. The sandy flats covered with efflorescent salts on this plateau represent, Mr. Streich thinks, depressions of the granitic uplands in which has been accumulated the saline matter remaining over from isolated parts of the ocean. In the north-western part of this plateau the Crystalline hills are capped with desert sandstone, which directly overlies the granite and is invariably horizontally bedded. Fossils were not found in this sandstone, which Mr. Streich considers to be probably identical with the similarly named formation of Central Australia.

[1] "The Geology of the Elder Expedition," *Transactions of the Royal Society of South Australia*, vol. xvi.

The granites wherever they outcrop bear a most distinct eruptive character, elsewhere they are overlain by rocks of Palaeozoic or Archean age, composed chiefly of hornblendic schists and slates in different varieties, and themselves overlain by feldspathic schists and quartzites of the same age, with talcose and micaceous schists and siliceous ironstone.

I am unable to add anything of the least value concerning the northern part of the district visited by me, and which lies beyond the country traversed by Mr. Streich; indeed, a fair knowledge of British secondary and tertiary deposits is a most inadequate preparation for effective study of coeval formations in Australia whose lithological characters are so different from those of European deposits. I will merely remark that what, judging from Mr. Streich's description, appear to be secondary rocks are to be met with in the country between Mount Flora and Lake Darlot, although in the absence of fossils I must candidly confess I considered these formations to be much older. What I have specially in memory are sandstones and conglomerates; and the so-called "breakaways" of the country in question correspond apparently with the terraced outcrops of Mesozoic rocks Mr. Streich found in his eastern section.[1] But Mr. Streich's observations suffice to give us an idea of the changes undergone by the southern part of the West Australian desert since earlier Cretaceous times. We may infer from them a westward extension, probably in the form of a wide arm of the cretaceous sea which divided Australia into an eastern and a western island, while during earlier tertiary times the eastern part of the desert would seem to have shared the fate of Central Australia, that is to say, that after having emerged from the waves, submergence again took place while the tertiary formations were being deposited. Whether this part of Australia was subsequently a lacustrine area or whether it was dry land, does not appear from the evidence, though the presence of desert sandstone near Yilgarn suggests the former condition. The western part of the desert was above water during Mesozoic times, and if the Darling conglomerates be Palaeozoic, a considerable area west of what is now the desert was also dry land during these times. In earlier tertiary times the district must, in its eastern part, have borne the character of an archipelago, and by subsequent upheaval the sea was divided into a number of inland salt lakes which gradually underwent desiccation.

[1] Altitudes were taken during the course of this expedition. The country east from Queen Victoria Springs is from 1000 to 1200 feet above sea-level, thence it descends to the Springs (830 feet), and rises west of it to from 1200 to 1450 feet. The highest point of the Fraser Range is 2010 feet above the sea, and the plateau to the west of the range, as has been already mentioned, 1300 to 1400 feet, while Mount Monger near Coolgardie is 1700 feet above sea-level. A plan showing the elevation of the country between the Darling Range and Mount Burgess, the work of West Australian government surveyors, was issued about three years back in connection with the proposed Coolgardie water-scheme. It bears out, in the main, so much of the above statement as concerns the country in question.

It will be well here to refresh the reader's memory by giving a short *résumé* of the ideas enumerated with such acumen by Mr. Wallace. The fact that this celebrated naturalist's conclusions respecting the geological history of Australia are faulty, should in no wise render us blind to the immense ability revealed in his brilliant pages, one's only regret concerning which is that the requisite data were not to hand when the work was undertaken. Mr. Wallace holds that at one period, perhaps during the middle or latter part of the secondary epoch, Australia was connected with land lying to the north, whence it received the ancestors of its Monotremes and Marsupials. As he points out, for such a connection the general level of the country would have to be raised at least by 6000 feet, and this would change the whole country, including the deserts of the interior as well, into a mountainous and well-watered region, and in such a region the rich and peculiar flora characteristic of the south-west (the Autochthonian flora of Professor Tate) was evolved. While the western flora was in process of evolution, Eastern Australia, if it had arisen from the ocean, must have been widely separated from Western Australia, so that the present continent then consisted of a large and fertile Western Island, and a long and narrow island stretching from far south of Tasmania to New Guinea, with one or more large islands to the north. A depression afterwards occurred which buried the greater part of North Australia beneath the ocean; whence it emerged in the middle or latter part of the tertiary period, and was stocked with vegetation from South-West Australia on the one hand, and from Indo-Malaya on the other. The flora of Eastern Australia has been derived from three sources: its south temperate element coming from Antarctic lands, the tropical element of Polynesian types from the north or north-east, and the typical Australian from across the dividing strait. Thus Mr. Wallace accounts for the "mixed" flora of Eastern, the isolated flora of Western, and the intermediate flora of Northern Australia.

There are several objections to these views of Mr. Wallace. One, the existence in tertiary times of a sea separating the eastern and western part of the continent, has, as we have seen, no warrant from ascertained facts of geology, neither is there evidence for the submergence of Northern Australia on a wide scale at the period when Mr. Wallace supposes it to have taken place. Moreover, unless the Mesozoic upheaval was accompanied by much differential movement, the upraised area would be converted, not into a mountainous region, but into a raised plateau; while if mountain ranges of the supposed height were formed at all, their disappearance, with the exception of some insignificant hills, from hundreds of miles of country is wholly inconceivable. Besides, unless the geological record be extraordinarily defective, the date of the introduction of Marsupials is too early, seeing that remains of those animals are, with one exception in

Tasmania (Eocene), not met with earlier than the Pliocene age.[1] Mr. Wallace, it will be observed, adopts the conventional notions based on present distribution to which objection has already been made. On this view, if an Australian genus or species has the Indo-Malayan facies and is found outside Australia, or is closely related to extra-Australian forms, it must have migrated into its present habitat; but the palpable errors into which Mr. Wallace has been led while formulating what he believes to be the true explanation of the case, may perhaps lead us to suspect that there is something wrong in the inference from present distribution whereupon his views are founded.

Professor Tate's conclusions are also based upon notions as to present distribution. He considers the Australian flora to be composed of two elements, an endemic and an immigrant. The endemic flora is of three kinds: Euronotian in the south and east, Autochthonian in the south-west, and Eremian in the desert. The immigrant flora has two constituents—an Oriental, dominant in the littoral tracts, but mixed there with typical Australian genera, and an Andean, restricted for the most part to the highlands of New South Wales, Victoria and Tasmania, and with this he includes north temperate forms, that is species characteristic of north temperate regions. The Autochthonian element was dismembered in Cretaceous times, and except for possible inter-communication with the Euronotian *via* the present Eremian region during the period of tertiary submergence, and perhaps, too, by means of land in the south now submerged, it has remained in a state of isolation. The Euronotian element was modified during early tertiary times by the irruption of a primitive cosmopolitan flora. The Andean element was introduced during a glacial period, and since then the Eremian flora has been developed from Autochthonian and Euronotian constituents, largely modified by an incursion of Indian types, while at the same time the Euronotian gained accessions from the Indo-Malayan province, although migrants have probably been received at all times since the specialisation of the flora of the Indo-Malayan province.

It will be observed that Professor Tate is not content with making Australia a sort of botanical dumping ground during recent times, but that he ascribes a migrant character to the primitive tertiary flora as well. Is there sufficient justification for this? The primitive tertiary flora makes its appearance to all intents simultaneously in various parts of the earth, in North America, in Europe, at Perim, in Borneo, etc., as well as in Australia, and we have no evidence in any of these cases as to its origin in one of these localities, and of its migration into others. There seems also no conclusive evidence that the western part of Australia was absolutely isolated from the eastern half during earlier tertiary times, and it seems incredible, unless the climate of Western has greatly differed from that of Eastern Australia, that a flora which flourished over such a wide area as we have indicated, shall

[1] Tate, "Inaugural Address," p. 37.

have failed when it encountered a region which, except for some fresh-water lakes, interposed no bar to its advance. Moreover, Professor Tate's generalisation is the more unsatisfactory, inasmuch as we know nothing about the tertiary flora of Western Australia.

The idea that the primitive tertiary flora was an immigrant one, so far as concerns Australia, must therefore be regarded as exceedingly problematical. The wide distribution of that flora seems to show that, no doubt with local variations, all the countries inhabited by it enjoyed an approximately similar climate, and it is surely no extravagant hypothesis that Australia played a commensurate part with other countries in the evolution of the flora. Certain forms were of extra-Australian origin, doubtless; but we are not justified in assuming that one part of the great area peopled by that curious flora was shut out from the drama of evolution and condemned to be a passive recipient of forms generated elsewhere.

The key to the problem before us seems to be in the recognition of the fact of there being two main elements in the Australian flora, one xerophilous, the other hygrophilous, and by applying the same classification to fossil floras, and regarding the bulk of the forms having a typical Australian facies as xerophilous forms; the disappearance from countries outside Australia of natural orders and genera now confined to or characteristic of it can be accounted for without assuming the possession of some natural superiority by one flora over another. Let us take the case of Europe, which, during Miocene and still more during earlier tertiary times, had a climate considerably warmer than it has to-day. Now if, under these circumstances, the country were open and included stretches of desert (and this is precisely the character Mr. Wallace[1] considers it had during the Miocene age), here would be conditions exactly parallel in some parts of Australia, particularly Queensland, to-day. And what do we find there? In the drier parts typical Australian species flourish, while species of Indo-Malayan facies predominate elsewhere. It is therefore probable that the species of European tertiary floras referred to Australian genera were, for the most part, dwellers in the desert patches, while the moister places were occupied, to a large extent, by forms adapted to the conditions there obtaining. That the climate of Europe gradually changed during tertiary times we know, not only because the floras indicate decreasing warmth until the cold Pliocene age arrived, but because the great upheavals during the mountain-making epochs must undoubtedly have affected, in a marked degree, the near annual temperature of the upraised districts and of the countries in their neighbourhood. The diminution of its temperature would have the effect of rendering Europe better fitted to herbaceous vegetation; it would, in fact, change it from what I have previously called a dendritic to a herbaceous zone, and thus would be set up a tendency

[1] "Geog. Dict. of Animals," vol. i. p. 117.

towards the elimination of xerophilous forms. But it is not certain that the xerophilous vegetation completely disappeared from South Europe and Asia Minor, for it may well be—to cite a few instances only—that some Chenopodiaceae, and species of *Helichysum* (and a fair number of these still survive in countries bordering on the Mediterranean), may actually be descendants from herbaceous members of the xerophilous flora, and when the present distribution of these genera is borne in mind, there is, it is submitted, at least some probability for this view.

An objector will, of course, ask why it is, if the theory above sketched be true, that we do not now find species of *Eucalyptus* and *Banksia* and *Dryandsa* flourishing in deserts north of the equator. These, he will remark, are precisely the places to which a xerophilous flora would retire for shelter when driven by stress of climate from its former homes. Undoubtedly it would do so, if the desert then existed, and if no stretch of sea interposed to cut off the retreat of xerophilous species. The available desert country reaches from the Atlas Mountains across Arabia into Baluchistan, but from this the Sahara must be deducted, since it was, till quite recent times, submerged beneath the sea, and until the nummulitic limestone emerged from the waves, the ocean in which that extensive formation was laid down would be an effectual barrier to migration. Since Eocene times, however, this barrier has not existed; but it is not clear that Arabia and the drought-stricken regions bordering on it were deserts at the time when the two floras, xerophilous and hygrophilous, were engaged in their life-and-death struggle. If Perim can be taken as a guide—and there is no reason why it should not be—there is every reason to believe that Arabia enjoyed, in Eocene times, a climate much like that of Europe; and all we have to suppose is that the same change went on there as in Europe, namely that the climate became more favourable to hygrophilous forms, which were thus enabled to eliminate their xerophilous competitors, and that desert conditions subsequently prevailed, and the absence of Australian genera from the great northern deserts is explained. That this explanation presents difficulties is not to be denied, for the elimination, at least of arborescent forms, has been so complete, we should have expected that at least some few forms would have been able to adapt themselves to the altered conditions.[1] Still the disappearance of these forms is scarcely more remarkable than is the disappearance of "Northern" genera such as *Quercus* and *Alnus* and *Salix* from a country which must have afforded them, one would imagine, many eligible stations where they should have been able to survive.

Let us now turn to Australia. And first, one must express a

[1] The Indo-Malayan and East Asian species of the Proteaceous genus *Helicia* may perhaps be cases of adaptation in the sense used above; so, too, East Asian species of such genera as *Drimys*, *Baeckia*, *Leptospermum*, *Leucopogon*, etc.

doubt whether Professor Tate does not estimate too highly the rainfall of Central Australia during the *Diprotodon* period. The evidence relied on by Professor Tate is, it will be remembered, of two kinds, biological and physiographical. Now if the districts where *Diprotodon* remains are found were to a considerable extent lacustrine, conditions might well have prevailed there essentially similar to those occurring now in Central and South-Eastern Brazil, where, during the dry season, when the *cerrado* country is completely parched, a rich flora, maintained by condensation of vapour during the early morning hours, flourishes in the river-valleys. The remains, therefore, of large Herbivora by no means prove that the whole country afforded subsistence to those animals; and although the existence of lakes warrants the conclusion that the rainfall was greater than it is at present, there may still have been many places adapted to xerophilous vegetation, and to that alone. As regards the physiographical evidence, we have to take into calculation the probable effects of a great lowering of temperature, even if the agency of ice cannot be invoked. Professor Tate and Mr. Watt,[1] after a careful examination on the spot, deny the evidence of ice-action, although to explain certain phenomena easily susceptible of such an explanation, they are driven to resort to a theory they themselves admit is "wild in the extreme." On the other hand, Professor Baldwin Spencer and Mr. T. M. Byrne,[2] as the result of a recent investigation, have no doubt of ice-action in Central Australia, though they decline to say at what period it was in operation. But apart from this, we have incontestable evidence for a cold climate in South Central Australia during past Miocene times, when the southern part of South Australia, as is well shown at Hallett's Cove, was glaciated. Now, under these circumstances, much of the precipitation falling on Central Australia would take the form of snow, which, during the summer months, would melt and thus release large bodies of water sufficient to cause a considerable amount of denudation. On the whole, therefore, we need not suppose that the rainfall of Central Australia during the period under review, although doubtless greater than at present, was so excessive as to prevent xerophilous vegetation flourishing side by side with hygrophilous, and contrasting Central Australia with Europe there is reason for supposing that while in the latter case conditions favouring a mixed xerophilous and hygrophilous flora were gradually changed in the interest of the hygrophilous element, in Australia the converse held, the tendency in favour of xerophilous forms continuing into the present day.

What was the climate of South-Western Australia during early tertiary times? Did the primitive tertiary flora flourish there as in other parts of the world? There is no reason to doubt that it did,

[1] "Report of the Work of the Horn Scientific Expedition," p. 70.

[2] Reported in *Nature*, vol. lvii. p. 495 (1898). A supposed Queensland case is also alluded to here.

provided the climate was suitable to a mixed flora. Unfortunately this is precisely the question to which no answer is possible in the present state of our knowledge, and it is greatly to be wished that steps should be taken to examine the lignite beds of the Fitzgerald River,[1] which would in all probability suffice to solve the problem. But whether the primitive flora in its entirety flourished there or not, it is submitted that the peculiar flora of the south-west can be explained on the assumption of a great difference in climate between the south-west and other parts of Australia, a difference dating either from Eocene (possibly late Cretaceous) times or from some period shortly after the Eocene. We have to suppose that long before the great plains of the central and eastern interior became desiccated, a considerable area in the south-west was already under the influence of drought. Westward of this dry district, which could have supported only a meagre flora, a somewhat more genial climate must have prevailed, but one favourable, except in isolated areas, to xerophilous vegetation. And if any difficulty is felt in adopting this view on account of the proximity of the ocean, it must not be forgotten that in the Shark Bay district desert conditions actually prevail up to the sea-coast, and moreover that, although southward of this district there is a fair annual rainfall near the ocean, the precipitation rapidly diminishes in amount at short distances from it. I saw many scenes of desolation in the interior, but they were almost equalled in the neighbourhood of York, barely sixty miles inland, in the early summer of 1894; even on the coast itself, periods of drought, during which non-xerophilous vegetation has but a slender chance of survival except in specially favoured localities, are frequent in the summer season. In such a country as this, then, I venture to believe the rich flora of the south-west to have been mainly evolved, and not, as Mr. Wallace supposes, in one diversified by lofty mountain systems, which, if they ever existed, have, except for the lowly Darling and Stirling Ranges, the upland districts of the far North-West, and the insignificant hills scattered over the vast intervening territory, vanished without leaving a trace of their former presence.

I cannot agree with Professor Tate in thinking that, except very rarely, there has been no interchange between the floras of Eastern and Western Australia. The flora of the Western desert has a fair number of species common to the two areas; there are also a considerable number common to the desert and the south-west, and some eastern species which advance to a greater or less distance into the desert, but without reaching the western coast region, and all this seems to indicate a filtration, most probably very slow, across the desert plains. It may also be remarked that recent discoveries have diminished the number of large genera having no species common to

[1] This and a thorough exploration of the North-Western territories are the two most interesting and important achievements now remaining to naturalists in Australia.

the two areas. But the evidence from species is not a satisfactory disproof of communication, for desert varieties are, as is well known, rather frequent, and if in the course of ages species have been differentiated from such varieties, a fair amount of concealed interchange may have taken place. The evidence is much stronger in respect of genera restricted either to the east or the west side of the country. Of these there are a large number which have not succeeded in making the passage, although many—xerophilous ones especially—have advanced some way towards doing so. These cases give emphasis to the conclusion that interchange across the desert has taken place very slowly, and to no considerable extent on the whole, although I cannot help thinking that Professor Tate decidedly underestimates its amount.

For long periods, perhaps since Cretaceous times, the evolution of the flora of Eastern and Western Australia has proceeded along different lines. So far I am in accord with other writers, and indeed this seems the only inference to be drawn from the facts. But the main reason for this is to be sought, as I venture to think, not in the simple isolation of the western part of the country while the eastern has been accessible to migrants from outside which have made headway against the endemic vegetation in consequence of their inherent superiority, but in climatic difference which had already become pronounced while the eastern interior was still a comparatively well-watered country. In short, I see Western Australia to-day supporting a vegetation similar, in its chief elements, to that which would now have been flourishing in Europe if our continent had been undergoing desiccation since Miocene times, and without lowering of its mean annual temperature. The interposition between the two halves of Australia of a sea and of a desert has, no doubt, laid an embargo on migration from one to the other. But for these barriers many restrictedly eastern forms would now be found upon the western seaboard, and *vice versa*. But we are not warranted in supposing that interchange would have taken place to such an extent as to result in a homogeneous flora; for the areas in Western Australia suited to hygrophilous forms are strictly limited, and the preponderating xerophilous element in the western flora is so well adapted to the extraordinary conditions prevailing in the west as to render its displacement in the highest degree unlikely.

Turning now to Eastern Australia, we find there a flora with little ordinal difference from that of the west, but containing many genera and a large number of species which, if they advance westward into the desert at all, do not reach the coast. Moreover, speaking generally, as we proceed northwards, and this applies generally to the moister regions near the coast, the number of forms possessed in common with Indo-Malaya and of forms allied to such tends to increase. There is also a sprinkling of species now characteristic of northern

lands and of species congeneric with these. A few of these species occur also in Western Australia; in the eastern districts they are more abundant in the cooler mundane country, and it is especially in the latter that are found forms conspecific with, or closely allied to, forms now forming part of the Antarctic flora, accepting this term in the conventional sense, that is, as embracing genera now largely or entirely restricted to southern cold temperate lands. It is admitted that these facts do, at first sight, favour the view of migration on a large scale followed by partial overpowering of the indigenous flora. But when we consider what is known of the history of Australia since late Cretaceous times the matter wears a different aspect. We have every reason to believe that since these times considerable portions of Eastern Australia have enjoyed a climate almost identical with that of Indo-Malaya, a climate, too, still prevailing in the north and north-east. We know, moreover, that in early tertiary times the floras of both countries were in a large measure identical. Is there anything remarkable, therefore, in the evidences of floristic affinity between the two regions? It will perhaps be conceded, as Professor Tate himself has conceded, that a certain proportion of the Australian species of genera common to these two neighbouring areas are descendants from the primitive flora, but that by far the larger number are immigrants. This, however, assumes our possession of complete records respecting the two floras from Eocene times to the present; and that we have anything like such records is an assertion no competent person would take upon himself to maintain.

But migration there has been, and the number of identical species and such a fact as the discovery of outlying forms allied to Indo-Malayan on the Bellenden-Ker range in North Queensland prove it, as also does the existence of "Australian" forms on the mountains of New Guinea, and in less number in various parts of Indo-Malaya and Eastern Asia, if, indeed, these last be not descendants from the primitive flora. Moreover, the trend of migration from the north has undoubtedly predominated over that from the south. But are we justified in assuming from this that any superiority is inherent in the Indo-Malayan flora over the Australian? What are the data? A number of hygrophilous genera and a certain proportion of hygrophilous species are common to the two regions. Now the Indo-Malayan flora, exception made for that of Timor, in some measure xerophilous, is and has long been a hygrophilous flora; while in Australia since Eocene times, if the view above enunciated be correct, hygrophilous types have had to struggle with xerophilous ones, which latter to-day still form a large element in its flora. Whatever in this case the means whereby migration has been brought about, its trend must, other things being equal, have borne direct relation to the size—or what comes approximately to the same thing, the comparative floristic richness—of the areas between which the interchange has been made. We have

also only to consider the hygrophilous element in the two floras, since the Indo-Malayan climate is not suited to xerophilous ones. While, therefore, the considerable areas in Northern and North-Eastern Australia[1] favourable to hygrophilous species have been open to the incursions of the whole of the rich Indo-Malayan flora, only those Australian forms adapted to hygrophilous have had a chance of penetrating into Indo-Malaya. It is submitted, therefore, that a preponderant migration from the north is only what ought to be expected on the doctrine of chances, and that there is no need to import into the discussion notions as to relative superiority and inferiority. We thus stand here upon precisely the same ground as that taken up in considering the supposed aggressive power of the Scandinavian flora.

The case is different with the Antarctic element of the Australian flora. This comprises forms suited to the lower grades of temperature, and all available evidence teaches us that colder conditions have been, of course in a geological sense, temporary only in Australia. But bearing in mind that glacial effects must have lasted a very long time, as contrasted with the span of human life, we may suppose that species of which the ancestors were received from the south may have been differentiated within the wide area in Australia suited to Antarctic forms during glacial times and times immediately preceding and following them, and that some at least of such species, accompanied by native ones which had become adapted to colder conditions, would migrate south when glacial conditions passed away, and so add a new, if small, element of Australian origin to the Antarctic flora. In any event, the Antarctic element seems to be an immigrant one.[2] I do not remember any attempt to prove from the presence of Antarctic forms the possession of "aggressive power" by the Antarctic flora, though, as the evidence for migration is so much stronger in this case, the omission, to say the least of it, is somewhat strange.

But Professor Tate tells us that a flora of exotic origin is in the act of displacing its indigenous vegetation from Central Australia. Let us see upon what evidence this conclusion reposes. Most of the truly Australian forms, he says,[3] usually grow gregariously or in isolated colonies from a few square yards to several square miles in area. But in a country like Australia, where good patches of soil alternate with bad ones, this gregarious habit scarcely implies want of adaptation. I saw precisely the same thing in Western Australia, and the inference I drew from it was directly contrary to Professor Tate's, namely, that the large numbers of a species monopolising or almost monopolising considerable portions of ground argued success in their

[1] And to a somewhat more limited extent the North-West too.

[2] Some of the herbaceous genera now characteristic of northern lands represented in Australia may have been introduced from the south during the glacial period.

[3] "Report of the Horn Expedition" (Botany), p. 120.

competition with other forms. Further, Professor Tate thinks the aggressive nature of alien plants to be exhibited not only by their extensive distribution, but by their ability to adapt themselves to extremes of soil and climate. He cites the following species in illustration: *Tribulus terrestris*, *Cleome viscosa*, *Malvastrum spicatum*, *Boerhaavia diffusa*, *Salsola kali*, *Mollugo hirta*, and *Pollichia zeylanica*. Now, even granting these to be aliens, and I think there are grave reasons for doubting the exotism of more than one of them, if these alien migrants from a hygrophilous zone are better adapted to desert conditions than native species which have had the advantage of long adaptation, it is strange that their distribution in the desert should be so restricted. Only four of the seven have been recorded from the western desert at all. I myself met with but two of them, viz. *Tribulus terrestris* once only, and *Salsola kali* about half-a-dozen times, but on only one occasion in any quantity.[1] Moreover, it should be remembered that these are all herbs of wide extra-Australian distribution, and provided, most of them, with special means of diffusion. Their presence in the desert is, therefore, easily explicable, and there is no warrant for drawing, as an upholder of current notions might wish to draw, from the fact any inference as to the superiority of an exotic flora over the native flora as a whole. On the gorges of the tablelands and on the basal part of the craggy escarpments and their taluses Professor Tate found a mixed flora in which the endemic element predominated, ten per cent only of it being of (supposed) exotic origin. The exotic species are seven in number; of these, except *Hybanthus enneaspirmus*, reported only from Mount Squires, on the eastern border of the western colony, *Parietaria debilis* alone has been found in the western desert. And when we compare the two lists above-mentioned, a curious fact comes out, namely, *that the name of not one species occurs in both*, and this forces one to suspect that Professor Tate has overestimated the adaptability of these supposed alien species. It would be wise, therefore, to reserve judgment on so difficult a point as that mooted by Professor Tate.

The view here taken up, it will be observed, is one intermediate between that of writers who, basing their conclusions on present distribution alone, profess to trace "currents" of vegetation from one part of the world to another, and ascribe the moving force, if the term may be allowed, of these currents to some natural inferiority of forms native to the country towards which the current is supposed to set—between this view and that of Baron von Ettingshausen,[2] who, while

[1] I should have been only too happy to come across *Salsola kali* more frequently, as it is an excellent fodder for camels. Doubtless it is much more common in South Australia, for on the occasion referred to above, our Afghan, who had worked at camel establishments in the eastern colony, at once recognised the plant, calling it "South Australian salt-bush," and informing me that it is an important fodder-plant there.

[2] "Contributions to the Tertiary Flora of Australia," *Mem. Geol. Surv., N. S. Wales*, 1888.

maintaining that the evolution of the existing floras from the tertiary flora was effected through differentiation of climate—and this, I venture to think, has been a main cause of floral diversity—dismisses as "absurd" the doctrine that certain floristic identities and affinities between regions now separated by the ocean are to be explained on the hypothesis of a former land-connection between them. I believe such land-connections to have existed, and, indeed, the present distribution of animals vouches for the truth of the theory. But until we know a great deal more than is at present known about the floras of any two countries previous to their being placed in continuity, I fail to see the possibility of estimating, except as mere guess-work, the respective effects upon the two floras so connected. Further, I believe the inferences from present distribution and from floristic superiority and inferiority upon which the current notions as to the origin of the Australian flora are founded, to be in the highest degree misleading. So facile a solution of the problem may commend itself to some; but it may be that many an observer will have to add his contributions of fact and suggestion before the final solution is reached; and if the notions here propounded, though they should fail to find acceptance, should at least drive home the conviction that much yet remains to be done in this fascinating field of research, the writer's object will have been fully attained.

# FRESH FACTS.

INFLUENCE OF COLD ON DEVELOPMENT. OSKAR SCHULTZE. "Ueber die Einwirkung niederer Temperatur auf die Entwickelung des Frosches," Zweite Mitteilung, *Anat. Anzeig.* xvi. 1899, pp. 144-152. Prof. Schultze published a communication on this subject in 1895, in which he stated that subjection to zero temperature brought the development of the eggs of *Rana fusca* to a standstill. Further experiments have, however, convinced him that this is not the case. Even at zero the cell-divisions continue, though more slowly. He has not been able to bring about a complete non-fatal standstill in the frog's development; if it is producible, it must be by temperature below zero.

A SNOW-WORM. J. PERCY MOORE. "A snow-inhabiting Enchytraeid (*Mesenchytraeus solifugus* Emery) collected by Mr. Henry G. Bryant on the Malaspina Glacier, Alaska," *Proc. Acad. Nat. Sci. Philadelphia*, 1899, pp. 125-144, 1 pl. A somewhat detailed account is given of the structure and habits of this worm, which has so remarkable a home. A very striking peculiarity is the yellow-brown, deep chocolate-brown, or almost black colour, and its opacity. Associated with it was a small Podurid, *Achorutes nivicola*, also black, and there are other instances. "It seems probable that some factor in a snowy environment lays the brand of melanism upon all the constituents of its invertebrate fauna." But "zoological literature fairly bristles with attempted explanations of melanism." The author discusses the physiological interest of an animal which lives and grows while maintaining a body temperature seldom varying much from the freezing-point of water.

FACTS OF INHERITANCE. ERNEST WARREN. "An observation on inheritance in parthenogenesis," *Proc. Roy. Soc.* lxv. 1899, pp. 154-158, 1 fig. Dr. Warren has made measurements of successive generations of *Daphnia magna*, which, though insufficient in number, "appear to favour the view that inheritance in parthenogenetic generations resembles that from mid-grandparent to grandchildren." "If this kind of inheritance be found to hold at all generally in parthenogenesis, it would be a fact of very considerable significance, and might conceivably give some insight into the physiological causes of heredity and variation."

HAS THE HAG A PARIETAL EYE? F. K. STUDNIČKA. "Zur Kritik einiger Angaben über die Existenz eines Parietalauges bei Myxine glutinosa," *SB. böhmisch. Ges. Wiss.* 1898 (published 1899), 4 pp. In one specimen of *Myxine*, Dr. Beard observed in 1889 a distinct parietal eye, but Retzius, Saunders, and Leydig sought for it in vain. More recently, Studnička has joined in the search, and is emphatic in declaring that there is no trace of the organ to be found.

THYROID AND THYMUS OF AMPHIBIANS. HERMANN BOLAU. "Glandula thyreoidea und Glandula thymus der Amphibien," *Zool. Jahrb.* xii. 1899, pp. 657-710, 11 figs. Two kinds of thyroid occur, one with colloid vesicles, the other with a connective tissue meshwork including leucocytes and blood-vessels. The number on each side differs in nearly related forms, but there is never more than one colloid gland on each side. In Ecaudata the gland is always colloid except in *Molge rusconii*. The thymus is single on each side in Ecaudata and Caudata, except in the larval form of *Amblystoma tigrinum*, which has a variable number. In *Siphonops*, as Leydig has shown, there are

four in a row. The content of the thymus is a connective tissue meshwork with included leucocytes, besides Hassall's corpuscles, and sometimes fine capillaries.

THE SAURIAN DIAPHRAGM. F. HOCHSTETTER. "Ueber partielle und totale Scheidewandbildung zwischen Pleurahöhle und Peritonealhöhle bei einigen Sauriern," *Morph. Jahrb.* xxvii. 1899, pp. 263-298, 1 pl. and 6 figs. The question has often been raised whether the mammalian diaphragm—the diaphragma dorsale—is a distinct and independent structure. Goette and others have contributed to the answer. In the paper before us Herr Hochstetter shows from studies of *Stellio vulgaris*, *Lacerta agilis*, and other lizards, that there are Anlagen present which furnish adequate basis for the evolution of the mammalian structure, and also for the very different diaphragm of embryo-birds.

GREEN PIGMENTS. MARION I. NEWBIGIN. "On the affinities of the enterochromes," *Zool. Anzeig.* xxii. 1899, pp. 325-328. While acid acts on an alcoholic extract of green leaves in such a way as to produce the pigment phyllocyanin, which is insoluble in alcohol and ether, its action on an alcoholic extract of green Algae results in the production of a pigment which is exceedingly soluble in alcohol, and does not therefore precipitate from acidified alcoholic solutions unless a considerable amount of water be added. In its colour and fluorescence, in its spectrum, in its changes in colour and spectrum on the addition of acid, in its solubilities, the pigment shows a remarkable resemblance to the enterochromes. This resemblance is such that, taken in conjunction with the recent observations and conclusions of Dr. M'Munn in the case of "enterochlorophyll," and with the fact that that pigment occurs in the faeces of *Patella*, it seems to justify the conclusion that "enterochlorophyll" at least is an acid derivative of chlorophyll, produced by the action of the digestive juices on the chlorophyll of the food. Whether the other enterochromes, and notably chaetopterin, are produced in the same way, cannot as yet be determined. There can, however, be no doubt that the enterochromes are at least closely related to the pigment produced by the action of acid on the chlorophyll of green Algae.

WANDERINGS OF WARBLES. P. KOOREVAAR. "The larval stage of *Hypoderma bovis*," *Ann. Nat. Hist.* iv. 1899, pp. 69-73. Translated by E. E. Austen from *Tijdschrift der Nederlandsche Dierkundige Ver.* v. 1898, pp. 29-34. Various experiments intended to throw light on the obscure corners of this life-history have led Mr. Koorevaar to the opinion that the young larvae of *Hypoderma bovis* at first pass beneath the skin; and thence betake themselves to the spinal canal and other places, to return later into the subcutis, and there undergo further development under the well-known conditions.

LUMINOUS ORGANS. LEOPOLD JOHANN. "Ueber eigenthümliche epitheliale Gebilde (Leuchtorgane) bei Spinax niger," *Zeitschr. wiss. Zool.* lxvi. 1899, pp. 136-160, 2 pls. and 1 fig. Brown or black spots on the skin of this fish turn out to be luminous organs. Their origin is like that of skin-glands; their elements are differentiated as luminous cells and lens-cells; their luminosity was observed by Dr. Th. Beer.

A STRANGE CREATURE BROUGHT TO LIGHT. CHARLES MINOR BLACKFORD. "A Curious Salamander," *Nature*, lx. 1899, pp. 389-390, 2 figs. [Letter]. From an artesian well, sunk 188 feet in limestone, near San Marcos in Texas, various white and blind crustaceans have been obtained. Even more striking, however, is a salamander, believed to represent a new genus and species. It has been named *Typhlomolge rathbuni*. It is from 3 to 4½ inches in length, and dingy white in colour, except on the external gills where the red blood shines through. The eyes are completely covered by the skin, but are seen from the outside as two black specks.

# SOME NEW BOOKS.

## PATHOLOGY OF PLANTS.

A Text-Book of Plant Diseases caused by Cryptogamic Parasites. By GEORGE MASSEE, F.L.S., Principal Assistant (Cryptogams), Royal Herbarium, Kew. Crown 8vo, pp. xii. + 460. London: Duckworth and Co., 1899. Price 5s.

THE announcement of a "Text-book of Plant Diseases caused by Cryptogamic Parasites," written by Mr. George Massee of the Kew Herbarium, naturally raised anticipations that the book would be distinctly better than any existing English work on the subject. The book now before us, on the whole, justifies the expectation, and is a welcome addition. The author's stated object is to give the inquiring grower of plants information on diseases caused by fungi and allied organisms. The introductory fifty pages give a general summary on the principles of preventive treatment, and the preparation and application of remedies. The greater part of the book (pp. 53–349) is occupied by short descriptions of the more important diseases. These descriptions are brief, yet clear, and free from unnecessary detail; they are well illustrated with figures mostly original, and the means of prevention are given in each case. The accuracy of this part of the work is guaranteed by Mr. Massee's long experience. The arrangement of the descriptions in the order of the fungi causing them is perhaps that least convenient to practical growers. The diseases described are selected—in a book of 450 pages this is essential—but the method of selection is vague. Only cultivated plants are considered, yet one sees no mention of several familiar British garden diseases, *e.g.* the smut and the rust on violets. Such omissions, and the complete neglect of the diseases of wild plants, many of which may easily attack cultivated forms, render the ambitious title of "Text-book" misleading. The inclusion of lengthy descriptions of diseases of exotic plants (*e.g.* tea and coffee) seems out of place in so small a book; it is doubtful whether the description of a few diseases will greatly assist growers in our colonies, while it introduces a confusion as to whether the disease under consideration is important in Britain, especially since this is not always clearly stated. On p. 349 the reader will find himself suddenly introduced into a maze of 100 pages of terminology peculiar to a fungus-flora. Reference to the "Contents" explains that these are "scientific descriptions of the fungi enumerated as causing diseases." Are these intelligible to many growers of plants? Are they, considering their limited number, of much use to the scientific worker? We venture to believe that an extension of the earlier parts of the book, and the omission of this portion, would have made it look more appetising to the practical man. Such defects can, however, be remedied, as also may some minor faults in typing and reproduction of drawings. The book is the work of the best investigator on the subject in Britain, it contains much valuable information in a readable form, its price is moderate; hence it is an indispensable addition to the library of the plant-grower who would learn as much about his subject as he can. W. G. S. (LEEDS).

## FERMENTATION.

The Soluble Ferments and Fermentation. By J. REYNOLDS GREEN, Sc.D., F.R.S., Professor of Botany to the Pharmaceutical Society of Great Britain. Pp. xiii. + 480. Cambridge University Press: C. J. Clay and Sons, 1899. Price 12s.

We can thoroughly recommend Prof. Reynolds Green's book to all who wish to obtain a trustworthy guide to a correct knowledge of the processes of fermentation. When we consider that it is little more than half a century since Pasteur discovered that the cause of alcoholic fermentation is the activity of the yeast plant, the strides made in our knowledge appear prodigious. There is hardly any aspect of animal or vegetable life in which ferments do not play some part. The ferments that cause our food to be digested, that produce clotting of the blood, that bring about oxidation, that minister to plant life in various ways, that are associated with the putrefactive and other changes wrought by bacteria, are all described with full details, and in a lucid, interesting manner; the history of the subject is also well given. The distinction between the organised ferments, like yeast and bacteria, and the soluble or unorganised ferments or enzymes, such as pepsin of the gastric juice, and diastase in germinating seeds, is a useful one. But the difference is more apparent than real, for probably in all cases the micro-organisms which are called organised ferments perform their work by secreting soluble ferments or enzymes. The recent work of Buchner has certainly shown this to be the case for yeast. What enzymes really are, and how they produce changes in large masses of material without any apparent change in themselves, or in their power, are much more difficult points to answer; the reader will, however, find in this book such general questions discussed in the light of recent knowledge. The ferments themselves, so far as any positive statement can be made, appear to be real chemical substances, and in their composition are allied to the albuminous bodies, particularly to the class known as nucleo-proteids. The interesting recent work of Emil Fischer, which is very clearly described, shows a possible way in which such substances could produce the change known as fermentation. All these theories are, however, tentative; whether they will stand the test of time, the future only can show. H.

## A MODEL FLORA.

The Flora of Cheshire. By the late LORD DE TABLEY (Hon. J. BYRNE LEICESTER WARREN, M.A.) Edited by SPENCER MOORE. With a Biographical Notice of the Author by SIR MOUNTSTUART GRANT DUFF. Pp. cxiv. + 399, with a Portrait of the Author, and a Map of the County. London: Longmans, Green, and Co., 1899. Price 10s. 6d.

Cheshire botanists will welcome the appearance of this flora of their county, though an interval of close on quarter of a century has elapsed since the preparation of the materials now for the first time printed. During such a period many changes must of necessity have taken place in details of local distribution, but the flora has been brought as far as possible up to date by the editor, and is a model of what a county flora ought to be.

The author has devoted great attention to describing, often at considerable length, the physical features, soils, etc., of the specific habitats, a point of great biological importance, and one too often overlooked. Duly authenticated, and, where possible, personal records are provided of the occurrence of each species in each of the seven hundreds, and a striking feature of the book is the care expended on the enumeration and history of alien and introduced species, many of which are traced back to their origin in ballast heap or garden, while numbers

of mere casuals are mentioned either as having been personally observed or recorded in the past. The flora proper is accompanied by an all too short physico-botanical account of Wirral (unfortunately the only hundred so treated), a short account of the Bucklow hundred, and a bibliography of Cheshire botany. J. A. TERRAS.

## TELEOLOGY.

Elemente der empirischen Teleologie. By PAUL NIKOLAUS COSSMANN. 8vo, 132 pp. Stuttgart: A. Zimmer's Verlag (Ernst Mohrmann), 1899. Price 4 marks.

There are some biologists who think, or who speak as if they thought, that teleology is a vestigial organ in culture—a way of looking at things which has had its day, and must gradually cease to be. Purposive structure and function —adaptation in short—they admit, but Darwinism has supplied the "mechanical explanation," and teleology is an irrelevancy. To others it seems that in biology we have not yet got very far in discovering the causal chains, the last link of which is an adaptation, and that even if we had got much further, we should have reached only a formulation in simpler terms. To these, teleology appears no irrelevancy, but a necessity of thought. Far from destroying teleology, Darwinism has rather deepened it.

The author of this book is an ardent teleologist, who seeks to show how difficult it is for us even in our scientific phraseology to get away from teleological conceptions, and how partial the outlook is which rests satisfied with chains of cause and effect. In working out these, the teleological idea is irrelevant and even inhibitive; for their development as parts of an intellectual system it is, however, necessary, since so-called scientific explanations are not explanations at all. The book is full of quotations and illustrations intended to show the difficulty of eliminating teleological conceptions from biology, and the utility of appreciating them. It might be described as a plea for a franker recognition of the purposive, and should be interesting to students of "Methoden lehre" and the philosophy of biology. X.

## A FALSE ANALOGY?

La spécificité cellulaire, ses conséquences en biologie générale. By L. BARD. Professeur à la Faculté de médecine de Lyon. (Scientia. No. I.) 100 pp. Paris: Georges Carré et C. Naud, 1899. Price 2 francs.

In the young organism, or young organ, there is often apparent uniformity among the component cells. As observed by our methods, they show no hint of the variety of cellular type which will gradually arise among their descendants. Many biologists have described this early state as one of "cellular indifference," and have ascribed the subsequent differentiation to the variety of cellular environment which ensues as the elements become more numerous. But this way of looking at the facts does not commend itself to Professor L. Bard, who has since 1885 been insisting on what he calls "la spécificité cellulaire." According to this view the various types of cell in the body are like different species with a common ancestor; one cannot be transformed into another; their differentiation is not a function of their environment, but an expression of their inherited properties. Virchow's famous formula has, he says, to be modified into "Omnis cellula e cellula ejusdem naturae."

He admits that his theory has not been welcomed by histologists, but he takes heart in detecting a gradual loss of confidence in the theory of cellular indifference. A final triumph, he tells us repeatedly, awaits his doctrine, and he has no patience with eclectics who would recognise that the early indiffer-

ence is only apparent, or that there is a certain degree of "spécificité." There is no middle way for Professor Bard: it must be yea or nay with spécificité.

In the first chapter of this little book he contrasts the conceptions of indifference and spécificité in a manner which appears to us exaggerated; in the second chapter (on the hereditary fixity of cellular types in adult organisms) he seeks to answer various objections which are suggested by the facts of cellular modifications, of regeneration, of heteromorphosis, etc.; in the third chapter he pursues the analogy of cellular species, and traces their establishment in the course of development; in the fourth he shows how his doctrine bears upon the general problems of biology; and finally there is a list of nineteen publications in which the author has previously dealt with the question.

The new series, of which this book is the first, has for its aim "l'exposé philosophique des faits généraux et des idées directrices nouvelles;" but though "la spécificité cellulaire" has evidently been a directive idea to the author, we do not think that he will succeed in convincing many that it is a general fact. To argue the question is not possible within our limits, and we can only express our opinion that the chief interest of the book is as an illustration of ingenious and enthusiastic special pleading in support of a false analogy. We may note in passing that there are a number of irritating misprints, *e.g.* Heckel for Haeckel, and Weissmann for Weismann. J. ARTHUR THOMSON.

## A PICTURE-GALLERY OF THE ISOPODA.

An Account of the Crustacea of Norway, with Short Descriptions and Figures of all the Species. By G. O. SARS. Vol. II. Isopoda. Bergen: published by the Bergen Museum. Sold by Alb. Cammermeyer's Forlag, Christiania.

We make no charge to other nations for the use of the English language. This generous extension of free-trade does not pass unrewarded. From time to time it brings us from abroad noble contributions to English scientific literature. It is in our own tongue that we have the satisfaction of reading "An Account of the Crustacea of Norway," by the Norwegian professor, G. O. Sars. His lifelong studies, embracing in turn the several groups of the crustacean class, have given him an almost incomparable facility and trustworthiness as an exponent of them all. The first volume of the "Account," which gave figures and descriptions of all the known Scandinavian Amphipoda, has already been reviewed in these columns. The intelligent reader, which is only another way of saying every reader of *Natural Science*, will recall something of what was then pointed out. It was to the effect that both the large agreement of the Norwegian fauna with our own, and the highly instructive handling of it by Professor Sars, made his work absolutely indispensable to every serious student of the Amphipoda in these islands. A similar remark may be applied to the second volume, just completed, which deals with a second order of sessile-eyed crustaceans, called Isopoda.

The name of this group was given it by Latreille in the Middle Ages, that is to say, nearly a hundred years ago, when people in general knew and cared about crustaceans hardly more than they now know and care about the centre of the earth. The name "isopod" signifies an animal with equal legs, and might therefore include most men and turkeys and many quadrupeds, though not so obviously applicable to the giraffe, the bison, or the kangaroo. But equality between legs, applying to two, or four, is less striking than when it refers to fourteen, a number with which the Amphipoda and Isopoda are endowed. In distinguishing the latter by the character of having equal legs, Latreille chose a name suitable enough to a woodlouse, such as *Armadillidium vulgare*, and to not a few of the marine species, such as *Sphaeroma serratum*, which, like the land woodlouse just mentioned, can roll itself into a neat little pill-like ball.

But since the time of Latreille there have been discovered in the depths of the sea and elsewhere, numerous species of Isopoda, in which the inequality and dissimilarity of the legs attached to one and the same body is carried to an almost extravagant extent. This will be seen at a glance by any one who only turns over the excellent plates with which Sars's work is illustrated throughout. The plates of the present volume are 104 in number. They would often be a kind of revelation to the casual observer, who seeing sees not, as he gazes at the animals themselves in the specimen glasses of a museum. The student also, preparing to dissect a rare isopod, should certainly first make himself acquainted with these drawings before attempting the severance of minute and delicate organs, which may be rendered undecipherable by one rash thrust of an ignorant needle.

As regards classification, it may be remarked that Professor Sars retains the Tanaidae among the Isopoda. This has the great merit of present convenience, whether or not at some future date the separatist party may succeed in detaching this group from its near allies. Most of the species of it, according to Sars, construct for themselves abodes of mud, into which they may wholly withdraw their bodies. On the English coast, however, there is one species pretty frequently to be found, along with the Gribble, in the honeycombing of submerged timber. On the group at large the Professor makes another observation, which is of much interest to the collector. "They all, moreover," he says, "exhibit this peculiarity, namely, that in reaching the surface of the water they remain floating, without being able to re-immerge their bodies, whereby the discovery of the generally very small and inconspicuous specimens is essentially facilitated. On placing some muddy clay taken from greater depths, in a shallow vessel, and stirring up the mud, they will very soon appear floating on the surface, like small white pins, and may easily be taken up for a closer examination." Naturally this mode of discovery will apply to the tenants of mud from small depths as well as great, and in some localities to the sand-dwellers of the sea-shore.

To the elucidation of the tribe Epicarida, it will be found that the work under review has made a very valuable contribution. In this tribe not Alps on Alps, but shrimps on shrimps arise. The Isopoda of which it is formed, in all sorts of insinuating ways, implant and engraft themselves upon other crustaceans, in the process assuming oddities of form, distortions and degradations, in pleasing but often extremely puzzling variety. There is plenty of work apparently still to be done in this branch of investigation, but the intricacies of it have been wonderfully disentangled by the labours, whether in conflict or agreement, of Giard and Bonnier, of Kossmann and of Sars.

In the great variety of species, normal and abnormal, which are shown to belong to the isopod fauna of Norway, it is singular that the Sphaeromidae find no mention. This is a family of extremely extensive distribution in the sea, and is represented even in fresh water. To one of the species incidental allusion was made at the beginning of this notice, simply because it is among the most familiar of British marine Isopoda, so that the absence of the whole family from Norwegian coasts and waters may well cause surprise.

The volume just completed is published by the authorities of the Bergen Museum. To them, therefore, as well as to the author, science is much indebted. There is one small but not unimportant improvement by which they might easily increase the obligation. The seven double parts of the original issue bear dates extending over four years, from 1896 to 1899. When the wrappers, which are of an essentially unstable character, are removed, the bound volume will contain the latter date alone. Since it teems from one end to the other with original observations, and with definitions and descriptions of new genera and species, the reader ought surely to be supplied with some means of ascertaining the true dates of its several parts. This could have been best effected by printing month and year of issue at the foot of the last page of the text,

and on the last plate, in each successive part. The object may be otherwise attained by supplying a continuous list of such pages and plates, to go with the preface or the index. Such a continuous list is in any case desirable, and might still be given for each of the volumes already published. The opportunity of distributing this small boon will be easily provided in company with a far greater one, the promised volume on the Cumacea of Norway, the appearance of which will be for its own sake eagerly welcomed.

THOMAS R. R. STEBBING.

## BUTTERFLIES' WINGS.

Specialisations of the Lepidopterous Wing: The Parnassi-Papilionidae. Parts I. and II. By A. RADCLIFFE GROTE. *Proc. American Philosophical Soc.* XXXVIII., 1899. Pp. 25-48, 3 plates.

The author's theory of the movement of the veins of the wings in specialisation suggests a guide for determining the systematic position of the genera with greater exactness, and a clue to their phyletic descent. The correctness of this theory of Grote's has recently received support through Dr. Rebel's discovery of an ancestral form of *Parnassius* from the Miocene of Gabbro, Italy. This extinct species, *Doritites bosniaski*, shows a neuration as yet in the zerynthian stage, and distinctly comparable with that of *Archon apollinus*, while the markings and facies are Parnassian. In this communication to the American Philosophical Society, the author reviews the genera of the Papilionides, showing the Parnassians to be the more advanced forms of the group, the test being the gradual disappearance, through absorption, of the cubital cross vein, as seen by an examination of the generic types from *Ornithoptera* up to *Parnassius*. He is led to the conclusion that the former genus shows generalised characters which bring it nearer to the presumed primitive Papilionid, and necessitate an alteration in the present systematic position of the genus. As opposed to the views of Spuler, the author repudiates any affinity between the Pieridae and Papilionides, and shows that the neurational analogies of the latter group lie with the brush-footed butterflies. The common white colour of the Pierids and Parnassians is ascribed to convergence, and reference is made to the author's earlier statements in *Natural Science*, that an increase of white pigment runs roughly parallel with the specialisation of the neuration. The author further considers and urges the probable diphyletism of the diurnals, as he has previously suggested, and recommends the retention of the Papilionides at the commencement of the series. The plates, in addition to the figures of Papilionides, give corrected figures of *Heliconius*, and for the first time of *Dione*.

## COLOMBIAN ORE.

The Ores of Colombia, from Mines in Operation in 1892. By H. W. NICHOLS, S.B. Field Columbian Museum, Publication 33. Geological Series, vol. i., No. 3, pp. 125-177, with Map.

This publication is a praiseworthy endeavour to utilise part of a collection made by Señor F. Pereira Gamba, a mining engineer of Bogota. The collection consists of specimens of the ores and associated rocks met with in those mines of the Republic of Colombia which were being worked in 1892. It was first exhibited in the World's Columbian Exposition, and about a quarter of it was subsequently handed over to the Field Columbian Museum, the remainder having apparently been lost. After an introduction, in which proper stress is laid on the circumstance that all of the specimens of ores are average samples, and after a couple of pages devoted to the physical features and general geology of Colombia, the serious business of recording, and in many cases describing,

the specimens begins. Preceding such descriptions there is in each case a preliminary historical account of the mining done in the district from which the specimens were collected. The sulphide ores are chiefly those of silver, zinc, mercury, lead, and iron, and occasionally of antimony, etc., while large amounts of native gold, silver, and sometimes copper are present in many of the districts. Among the sulphides pyrites is very common, and this is sometimes auriferous. Occasionally copper pyrites, mispickel, stibnite, cinnabar, tetrahedrite, pyrrhotite, enargite, etc., are present. The gold is frequently associated with tellurides. Many interesting examples of paragenesis are given, but to enter into details would be wearisome to the general reader, although they might be perused with avidity by those interested in the mines of this republic, while the mineralogist could not fail to find some useful information in them. One of the most important points with which the author has dealt is the true signification of the names of rocks hitherto employed by former writers when describing these mines. For instance, for the old terms syenite and granite, the author points out that one may generally read andesite or trachyte; "hornblendic material" usually is found to be a rock allied to chlorite schist, and several other examples of the former misapplication of names, owing to lack of the present means for determining the mineral constitution of rocks, will be found in these pages. The author has done useful work in solving some of these enigmas.

In the "General Conclusions," p. 172, he remarks that "the gold and silver ores of Colombia occur either in the acid lavas, which have been erupted at intervals from the close of the Tertiary to the present time, or in Archaean schists in the immediate vicinity of the lavas. In the schists they are usually poor in depth. Owing to the action of the heavy tropical rains, the weathered zone of the deposits has often been greatly enriched, and it was such enriched deposits that gave the immense yields of the early days of Colombian mining."

Three pages, giving the literature relating to Colombian mines, are followed by a map, on which considerable labour has evidently been expended in order to render the topographical details trustworthy. F. RUTLEY.

## COCCIDOLOGY.

The Coccidae of Ceylon. By E. ERNEST GREEN. Part I. 1896; pp. i.-xi. + 103, with pls. 1-30. Part II. 1899; pp. xiii.-xli., 105-169, and pls. 31-60. London: Dulau and Co.

The Coccidae constitute an aberrant group of the Hemiptera, contradicting all ordinary definitions of the order and class to which they belong. Hemipterous hexapods, yet in the female sex wingless, and in many genera legless as well. The very methods by which they must be studied are peculiar, and as such distasteful to the ordinary entomologist.

So it has happened that these creatures, though numerous and peculiar, have been greatly neglected. But in recent years, as though outraged by such persistent scorn, they have risen in their might and played havoc with our fruit trees and other crops, not to mention ornamental plants; wherefore we have been obliged to recognise their existence.

Studies usually begun with economic ends in view have led us far afield. It becomes plainer every day that the Coccidae are not only extremely numerous in species, but offer an extraordinary series of peculiar forms, whose organisation, as related to their environment and habits, is of the greatest interest from a purely biological standpoint. The opportunity to advance both economic entomology and pure science is too good to be neglected once perceived; and so we find a new body of students arising, calling themselves coccidologists, and dignifying their study by the name of coccidology.

Of these latter-day students assuredly E. Ernest Green is second to none.

Beginning his researches during the previous decade he at first proceeded slowly. The literature of the subject was difficult to obtain, and when obtained threw little light on the almost unknown coccid fauna of Ceylon. But Mr. Green, not discouraged, resolved to study every species *de novo*, whether described or not; acquiring his knowledge first-hand from nature, as though he might be Adam in the Garden of Eden. This method, in the hands of an intelligent worker, is sure to be successful, and it was eminently so in the case of Mr. Green. He not only prepared descriptions, but also elaborate drawings of every species in all its stages, so far as they could be obtained. This done, a thorough examination of the various publications on Coccidae had to be made before the apparently new forms could be reported as such; but this revealed comparatively few identities. In Part I. there are thirty species described, of which seventeen were new; in Part II. are twenty-nine species, all but three discovered by the author. It often happens, on receiving a paper describing so many new species, that one can immediately detect some synonymy; but I do not know of any "bad species" among the forty-three above mentioned, and doubt if there are any. The descriptions are good, and the plates most beautiful. There is an introductory portion on the general principles of coccidology, including a new classification of the sub-families and full directions for collecting and preserving material. There is also a chapter on the insecticides and other means for destroying Coccidae which injure cultivated plants. In the last-mentioned chapter the interesting fact is brought out that practically all the injurious coccids in Ceylon are those described from elsewhere and presumably introduced into the island. The truly native species, almost without exception, have proved to possess no economic importance; though of course these very species, *carried somewhere else*, may yet become notorious.

An unfortunate conservation, as it seems to the present writer, is shown in regard to genera. The species assigned to *Aspidiotus* represent at least five very distinct groups, which are at least of sub-generic value. *Aonidia* is made to include very diverse forms, including three distinct generic types. Similarly the twenty-six species assigned to *Chionaspis* are by no means truly congeneric; for instance the first six, *aspidistrae*, *theae*, *albizziae*, *mussaendae*, *rhododendri*, and *scrobicularum*, belong properly to *Hemichionaspis*. The generic classification of the Coccidae, however, is at present in a transitional state, and an author cannot be blamed if he hesitates to propose changes while yet uncertain what those changes should be.

Simply as an illustration of good methods this work ought to be examined by zoologists who do not expect to study Coccidae; while for the coccidologist it is of course essential. Under these circumstances it is to be regretted that the manner of publication is such as to make it extremely costly. The only edition has coloured plates, which of course are expensive, while the colouring does not greatly add to their value for scientific purposes. It would have been excellent to have a coloured edition of small size, if there could have been an uncoloured one at a more moderate price. But the chief trouble is that the publishers insist upon receiving the full subscription (£5) for the work in advance, though it must take at least several years to complete it. The work, of course, is intrinsically worth all that is asked for it, and more; but the fact remains that zoologists are not commonly blessed with superabundant means, and are reluctant to part with a five-pound note under the circumstances just mentioned. Surely if the conditions of sale were rendered easier the subscriptions would become so much more numerous that the amount received would be considerably greater than at present.

T. D. A. COCKERELL.

## MULTUM IN PARVO.

Insects: their Structure and Life. A Primer of Entomology. By G. H. CARPENTER. Small 8vo, pp. viii. + 404, with 183 illustrations. London: J. M. Dent and Co. Price 4s. 6d.

In this book the author traverses most of the very wide field of the division of zoology he is dealing with. Although the great extent of his subject prevents him from discussing moot points in detail, yet he succeeds in giving a very fair general idea of the present state of entomological science and of the subjects that have been predominant in the discussions of the last twenty-five years.

If any fault is to be found with the book it is that its subject has been extended unduly by the inclusion of matter that is not specially entomological. The chapter on the classification of insects is chiefly devoted to natural selection, causes of variation, and kindred topics. These matters are, however, set forth in a spirit and manner that no one can object to; and, though their predominance is scarcely consistent with the title of the work, it is probable that one of the author's objects was to show the bearing of entomology on these more general subjects. We hope that the work will find many readers, and that most of them will approve the wide view the author has taken of his subject.

The book is copiously illustrated and well got up. Most of the 183 illustrations, called figures, are really combinations of numerous figures. They have been well selected, and their execution, except in a few cases, is satisfactory. There is also a very useful and sufficiently extensive bibliographic list, and good index. Altogether, the work may be strongly recommended to purchasers who wish to give only a small sum for a trustworthy introductory work on this subject. They will receive excellent value for their money. D. S.

## THE MAMMALS OF FRANCE.

Faune de France—Mammifères. By A. ACLOGUE. 12mo, 84 pp. Paris, 1899.

To treat adequately of the mammals of France within the compass of eighty-four duodecimo pages, especially when a large portion of the space is occupied by introductory matter and illustrations, would seem an almost impossible task. Nevertheless, with the assistance of irritatingly minute type, and much "boiling-down" of matter, Monsieur Aclogue has succeeded not only in enumerating all the species, both wild and domestic, met with in France, but also in giving the leading characteristics of both genera and species, as well as of the larger groups.

Nothing, however, is said as to habits, and but little in regard to the details of local distribution and variation. And as the two latter features are those alone which would render the work of importance to zoologists of other countries, it can scarcely be said to be altogether satisfactory.

As it would appear from the title that the complete work is intended to include the entire fauna of France, the author may perhaps be induced to pay more attention to these points in subsequent parts. The character of the illustrations might, too, be improved with advantage. And there is likewise room for some amendments in nomenclature; *Arvicola*, for instance, being retained for the voles, while the martens figure as *Martes* in place of *Mustela*. Still, in spite of its imperfections, it is useful to have a work containing all the representatives of the French mammalian fauna.

## A CLASSIC FOR CLIMBERS.

Hours of Exercise in the Alps. By JOHN TYNDALL, LL.D. Pp. i.-xii. and 1-482, with seven full-page illustrations. New Edition. London: Longmans, Green, and Co., 1899. Price 6s. 6d.

This new edition of a highly characteristic work is practically the reprinting of a classic. So far as the compositors' work could allow, it is more—it is an actual reproduction. A full index has been wisely added, a matter on which the author was strangely indifferent; even his popular "Forms of Water" appeared without one. A few notes by L. C. T., bringing certain statements up to date, have been made with conscientious care.

The book was first issued, by the same publishers, in 1871, and must have been written with as much enjoyment as it has again and again brought to others. The human personality of it must always remain fresh; and climbers will not tire of details of these earlier exploits. Switzerland, the middle level occupied by church-congresses, university-extension parties, and the host of unattached or exploited tourists, has changed conspicuously in the last thirty years; but the great peaks and snow-girt amphitheatres remain for the men of firm nerve, of resolute and confident resource. Such men, year after year, bring to the snow-slope and the *arête* the quickness of judgment and the orderly perception which have made them masters in their own professions, masters alike of human prejudice and of mountain-barriers. Whether all such will approve the school-boy rashness of some of Tyndall's joyous escapades, none can fail to respond to his enthusiasm, or to smile with him in his hours of success. The story of the rescue of the porter on p. 144 touches a far graver note. The book concludes with several short papers, among which is a considerable discussion on regelation. On p. 421 we have the well-known account of a winter ascent of Snowdon, in the company of Professor Huxley. Though a number of Alpine climbers have since exercised themselves on the crags of Lliwedd and Cribgoch, how many of the English tourists who throng Grindelwald or Zermatt have seen Wales under any other covering but that of August rain? A journey from London to Llanberis, in the crisp clear days of January, will soon make even an ordinary walker share the enthusiasm of our author.

To say "our author" is not in this case a convention; there is much in this volume, even in its simplicity and candour, which must seem to all of us like the cheery handshake of a friend. G. A. J. C.

## EXPERIMENT IN GEOLOGY.

La Géologie expérimentale. By STANISLAS MEUNIER. Pp. i.-viii. and 1-312, with 56 illustrations. Paris: Felix Alcan, 1899. Price 6 francs.

This compact little book, forming a volume of the "Bibliothèque scientifique internationale," summarises its author's researches, which have extended over thirty years, much as those of M. Gaudry were aptly brought together in "Les Ancêtres de nos animaux." The author, possibly from a desire for dispassionate exposition, has chosen to write in the third person. This is the very opposite of the method of the late Professor Tyndall; and the middle tone of partial self-suppression adopted by authors of average literary gifts probably represents the canon of taste in dealing with one's own observations. Mr. Stanislas Meunier's plan has the disadvantage of reminding us of the handbill issued by Mr. Samuel Gerridge in Robertson's comedy of manners. Apart from this, the descriptions are of course clear and definite, in the admirable fashion of French text-books; and the discussions that are involved, as well as the replies to criticism, are never unduly extended. The instruments devised, and the permanent results obtained, have been formed into a collection in the

geological gallery of the Jardin des Plantes in Paris. The book thus provides an agreeable guide to this collection.

There is much in it that will be of service to the teacher of ordinary classes, such as the reproduction of earth-pyramids, described on p. 40, and of sand-dunes, described on p. 210; while the broad and at times generous deductions from the experiments deserve the attention of the physical geographer as well as the geologist. The discussion (pp. 107-111) of the continuous diminution of glaciers by the continuous erosion of their gathering-grounds and of their beds, is an example of how the larger natural features are always present to the mind of the experimenter.

The book is brief; yet there is frequent mention of the work of other authors. It is impossible, in such limits, however, that such reviews of previous observations should be complete. As an expression of Mr. Stanislas Meunier's own work and of his own conclusions, the volume is especially profitable to the reader. Towards its close, we touch on the great questions of igneous magmas and metamorphism, which are now agitating the geological world. We commend the bold suggestion made on p. 266, to those who regard the solution of one rock in another as confined to contact-phenomena. The author here derives the water required for volcanic action from the absorption of blocks of the water-logged outer layers of the crust by the molten and anhydrous mass below. G. A. J. C.

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We have received *Naturae Novitates* for 1898, that useful fortnightly bibliographic bulletin of natural science issued by Messrs. Friedländer of Berlin. The collected parts for 1898 amount to 780 pages, the index occupies about 90, the number of citations is 9359, and the price is only four marks. It is now in its twenty-first year of issue, and deserves to be congratulated on attaining its majority.

In the September number of *The Naturalist* there are obituary notices of Mr. John Cordeaux by W. Eagle Clarke and the Rev. E. A. Woodruffe-Peacock. A note by E. Whitehouse points out that *Hydra viridis* devours Aphides greedily. "The Hydra would thus be very serviceable in a greenhouse if they could live on plants."

The *Zoologist* for August 15 contains an obituary and portrait of the late Sir William Henry Flower, and a continuation of Mr. W. L. Distant's lively paper on mimicry.

*Knowledge* for August contains a continuation of the anthropological studies by Prof. Arthur Thomson of Oxford, and the tenth instalment of Mr. Stebbing's "Karkinokosm," which reads like a novel. A striking photograph of proboscis and snub-nosed monkeys illustrates a lively paper by Mr. Lydekker, entitled "A Contrast in Noses."

The *American Naturalist* for August contains articles on the Hopkins Seaside Laboratory, by Prof. Vernon L. Kellogg (*see* "Notes and Comments"); on the North American arboreal squirrels, by Mr J. A. Allen; and on an abnormal wave in Lake Erie, by Mr. Howard S. Reed. There is also an obituary of Dr. Alvin Wentworth Chapman, by Prof. W. Trelease, and a synopsis of North American Gordiacea, by Dr. Thomas H. Montgomery, jun.

*Knowledge* for September contains, *inter alia*, a fifth paper on the Mycetozoa, by Sir Edward Fry; a popular essay on Fairy Rings, by Mr. A. B. Steele; the beginning of an account of Ben Nevis and its Observatory, by Mr. W. S. Bruce; a paper on Clouds (with good photographs), by Messrs. E. M. Antoniadi and G. Mathieu; and a letter by Dr. C. S. Patterson adversely criticising some of the conclusions in Prof. M'Intosh's "Resources of the Sea."

In a short paper contributed to the *Mt. Ges. Bern* for 1897, Dr. A. Girtanner describes a fine piece of horn-cores of the European Ibex obtained from the pile-village of Greny on the Martensee. It appears that in cavern deposits, the farther we depart from the Alps the rarer become the remains of the Ibex, and hitherto the horn-cores have only once been found in a Swiss lake-village. From these facts it has been inferred that the animal was always a mountain-dweller. In the present instances the author compares the ancient horn-cores with modern horns, much to the disadvantage of the latter.

*Science* for September 1 contains the following interesting note:—"The American word 'scientist,' proposed by the late Dr. B. A. Gould, is apparently becoming acclimatised in Great Britain. Though *Nature* has stated that the word is excluded from its columns, it has occurred in the editorial notes. It will also be found in the *Academy* and in the London *Times*. The latter, in the issue of August 15, even uses the word retroactively, speaking of 'the great German scientists of the past.' But the best testimony that the word must now be regarded as correct and classical English, is the fact that it is to be found in Mr. Thomas Hardy's 'Two on a Tower.'"

The *Photogram* for July has a translation from *Le Photo Gazette* of an interesting brief article by Fabre-Domergue on photographing aquaria by magnesium flash-light. He experimented at the laboratory of marine zoology at Concarneau, and got some good results, a specimen of which is given. The magnesium light produces a lively effect on the fishes in the aquarium, but the reflex movement is relatively slow, and the light is gone before it takes place.

We have received the September number of *The Westminster Review*, which contains two articles involving biological considerations—one against the Contagious Diseases Acts, by Ellis Ethelmer, and another containing a suggestion of a substitute for the marriage laws, by Herbert Flowerdew. The latter says: "Let us suppose, then, that the legal marriage contract consists simply in an agreement between man and woman to live together until such time as either chooses to terminate the agreement, and to be jointly responsible for the maintenance of the children born during the arrangement and within nine months of the termination, both parties agreeing to compensate the other for any loss incurred by his or her failure to make the arrangement permanent."

The September number of *Science Gossip* contains, among other articles, one by Major H. A. Cummins on Sikkim, "a veritable paradise for the naturalist, be he botanist, zoologist, or geologist, but especially for the botanist"; and a continuation of papers on British freshwater mites, by C. D. Soar; on the collection and preparation of Foraminifera, by A. Earland; on ticks and louping-ill, by E. G. Wheeler; on palaearctic butterflies, by H. C. Lang; on chalk, by E. A. Martin; and on meteorites, by John T. Carrington.

Prof. L. V. Pirsson of Yale succeeds the late Prof. Marsh as an editor of the *American Journal of Science and Arts*.

The journal of the Straits branch of the Royal Asiatic Society for June 1899, which has been sent to us, contains some interesting papers. The list of birds of the Larut hills, by Mr. A. L. Butler, has some interesting field notes. Bishop Hose gives a list of the ferns of Borneo, and Mr. H. N. Ridley a list of the scitamineae of the Malay Peninsula. A pleasant little journey into an unexplored corner of Pahang is graphically described by Mr. W. B. Roberts. The most important paper, however, is one by Mr. Ridley on the habits of Malay reptiles. It contains many valuable observations which it is to be hoped will find their way into our books on natural history. The society may be heartily congratulated on the production of this volume, which indicates a considerable activity in biological work in the Straits Settlements.

# OBITUARIES.

## JOHN CORDEAUX

BORN, 1831; DIED, AUGUST 1, 1899

JOHN CORDEAUX was the eldest son of the Rev. John Cordeaux, formerly rector of Hooton-Roberts, Yorkshire. He was born at Foston Rectory, in Leicestershire. The ordinary occupations of a country life gave him opportunities for the especial study of bird life, with which his name has been so long associated. Among his earliest publications was one on the birds of the Humber district, which has remained the standard for that district. Ever keen on the migratory habits of birds, he strove hard to interest the coast-guard and lighthouse-keepers to keep continuous records at their various stations, and succeeded in obtaining a great variety of useful and interesting data, both on the migratory habits of our own birds and the occasional visits of strangers. Nor did he neglect other interests, for he was an enthusiastic botanist, archæologist, and student of folk-lore. Cordeaux was first president of the Lincolnshire Naturalists' Union. We are indebted to the *Times* for the facts in the above notice.

The following deaths are announced:—The entomologist PEREZ ARCAS in Madrid; Dr. DANIEL GARRISON BRINTON, the American ethnologist, at Atlantic City, New Jersey, on July 31, at the age of 62 years; the botanist EUGÈNE GONOD D'ARTEMARE, at Ussel (Corriège), on June 16; the psychologist FREIHERR KARL DU PREL, who made a number of contributions to evolution-literature, at Heiligkreuz in the Tyrol, on August 5, at the age of 60; Dr. PASQUALE FREDA, director of the agricultural experiment station at Rome, on July 4; Mr. N. R. HARRINGTON, member of the Senff Zoological Expedition, instructor in Western Reserve University, at Atbara, July 27; CHARLES HOWIE, of St. Andrews, who published a list of the mosses of Fife and Kinross; ST. TH. JAKČIČ, professor of botany and director of the botanical garden at Belgrad, on May 4; the French geologist ADOLPHE LEGEAL, murdered in the Soudan; Dr. JOSEPH MIES, anatomist and anthropologist, on June 9, in Köln; A. DE MARBAIX, professor of zootechnic at the Catholic university of Louvain, at Meerhout, near Antwerp, on August 5, in his 74th year; ROBERT H. SCHMITT, geographer in German East Africa, on May 10, in Mangali, at the age of 29; the mycologist JOHANN N. SCHNABL, in München, on June 16, at the age of 45; HENRI LÉVÈQUE DE VILMORIN, a notable cultivator of plants, first vice-president of the Paris Société d'Horticulture.

# NEWS.

THE following appointments have recently been made:—Dr. A. P. Anderson, assistant professor of botany in the University of Minnesota; Dr. O. Appel as an assistant in the biological department of the Agricultural and Forestry Institute in Berlin; Mr. Carlton R. Ball, assistant in the division of agrostology, U.S. Department of Agriculture; Dr. W. Benecke, as docent for botany at Kiel; Dr. Alfred Bergeat as professor of mineralogy and geology in the Mining Academy at Clausthal; Mr. H. Blodgett, an assistant botanist and entomologist in the New York Branch Agricultural Station at Jamaica; Dr. Franz Boas, as professor of anthropology in Columbia University; Prof. E. A. Burnett, of the Agricultural College of South Dakota, to the chair of animal husbandry in the University of Nebraska; Dr. A. Cancani, seismologist, to succeed Dr. G. Agamenone as assistant in the central office of meteorology and geodynamics at Rome; Dr. Chatin, as professor of histology at Paris; Prof. Alessandro Coggi of Perugia, as professor of zoology in the University of Sienna; Dr. N. K. Czermak, as professor of anatomy, histology, and embryology in the University of Dorpat; Mr. H. N. Dickson, as lecturer on physical geography in the New School of Geography in the University of Oxford; Dr. von Elterlein, docent for mineralogy and geology in Erlangen; Dr. Enrico Festa, assistant in the zoological museum of the University of Turin; Prof. Max von Frey of Zürich to be professor of physiology at Würzburg; Dr. Alberto Fucini, as docent for palaeontology and geology in the University of Pisa; Dr. Ercole Giacomini of Sienna, associate professor of zoology in Perugia; Mr. A. W. Gibb, to be university lecturer on geology in the University of Aberdeen; Dr. Hugo Glück, docent for botany in the University of Heidelberg; Dr. Guitel, as adjunct professor of zoology at Rennes; Mr. Eustace Gurney of New College has been appointed to the Oxford University table at the Naples Station; Dr. W. H. Hobbs, assistant professor of mineralogy and petrology at the University of Wisconsin, to a full professorship; Dr. A. C. Houston, as lecturer on bacteriology at Bedford College, London, for women; Abel A. Hunter, as botanical collector for the University of Nebraska; Dr. A. Jakowatz, as demonstrator in the botanical museum in Vienna; Dr. J. Janse, as director of the botanical gardens at Leiden; Dr. K. Keissler, as assistant in the botanical museum in Vienna; P. Beveridge Kennedy, assistant in the division of agrostology, U.S. Department of Agriculture; Dr. Max Koch, as titular professor of geology in Berlin; Dr. H. B. Kümmel, assistant professor of physiography at Lewis Institute, Chicago, has been appointed assistant state geologist of New Jersey; Dr. Künstler, as professor of comparative anatomy and embryology at Bordeaux; Dr. A. C. Lane, to be state geologist of Michigan; Albert Lindström, as honorary professor of anatomy in the Karloninian Institute in Stockholm; Dr. Günther Beck von Mannagetta, as professor of systematic botany in Prag; Dr. Johannes Meisenheimer, as privat docent in embryology in the University of Marburg; Elmer D. Merrell, assistant in the division of agrostology, U.S. Department of Agriculture; G. T. Moore, as instructor in botany at Dartmouth College, U.S.A.; W. A. Orton, assistant in the U.S.

Department of Agriculture; A. J. Pieters, as first assistant in botany in the Department of Agriculture at Washington; Dr. Antonio Porta, to be assistant in the zoological museum of the University of Parma; Prof. C. S. Prosser, of Union College, Schenectady, New York, as associate professor of historical geology at Ohio State University, Columbus; Dr. Francis Ramaley, of the University of Minnesota, to be professor of biology in the University of Colorado, at Boulder, in succession to Prof. John Gardiner, retired; Dr. A. L. Rimbach, as *ad interim* instructor in vegetable physiology and pathology in the University of Nebraska, to allow Prof. Bessey to give the necessary time to the duties of acting chancellor; Dr. Friedrich Schenck, as professor extraordinarius of physiology in the University of Würzburg; Hermann von Schrenk, as a special agent of the Department of Agriculture at Washington to study diseases of trees; James Y. Simpson, M.A., B.Sc., lecturer on natural science in the Free Church College, Glasgow; Dr. Otto Stapf, as chief assistant in the Kew Herbarium; H. G. Timberlake, as instructor in botany in the University of Wisconsin; Dr. Weinschenck, as docent for mineralogy and geology in the Technical School of München; Dr. Karl Wenle, as directorial assistant in the ethnological museum in Leipzig.

The Council of the University of Melbourne will shortly appoint a professor to the chair of Geology and Mineralogy. The professor is expected to devote the whole of his time to the work of his department, and will be required to deliver two courses of lectures of three hours a week each, and to undertake the training of students both in field and laboratory work. The salary of the professor is £1000 per annum, but in the event of the Council providing him with a residence in the University grounds, the sum of £100 per annum will be deducted from his salary as aforesaid. The University has a fair collection in palaeontology and mineralogy, but has no specially fitted up laboratory for geological work. A suitable room in the University buildings will be provided in which to organise this part of the work. Lectures begin in 1900, on Thursday, March 1. The salary of the office will commence from the 14th February 1900, or from the date of the professor's arrival in Melbourne, if later than the 14th February. If the professor appointed come from Britain or America, £100 will be allowed for travelling expenses. Applications for the vacant chair must be sent to the office of the Acting Agent-General for Victoria, 15 Victoria Street, Westminster, London, by October 20, 1899.

At a meeting of the General Committee of the British Association on September 15, Professor Sir William Turner was elected president of next year's meeting of the Association, to be held at Bradford.

A. Targioni-Tozetti has been elected an ordinary fellow of the Reale Accademia dei Lincei in the Department of Agriculture; A. Borzi as a corresponding fellow in the same department, and F. Delpino for botany. The list of foreign fellows includes—for geology and palaeontology, O. Torell, A. de Lapparent, and R. Lepsius; for botany, W. Pfeffer; for zoology, Ernst Haeckel and E. van Beneden; for physiology, E. Pflüger and E. Hering—a notable list, but without any British representative.

Prof. E. Pflüger, the famous physiologist of Bonn, recently celebrated his 70th birthday.

Prof. C. J. Herrick of Denison University has been awarded the Cartwright prize of $500 by Columbia University for his work on the brain of fishes.

The Académie Internationale de Géographie Botanique has conferred its international scientific medal upon Prof. John M. Coulter.

It is noted in *Science* that the Alvarenga prize of the College of Physicians of Philadelphia has been awarded to Dr. Robert L. Randolph of Baltimore for his essay entitled "The Regeneration of the Crystalline Lens: An Experimental Study."

On the occasion of the ninetieth anniversary of the foundation of the University of Berlin (at the beginning of August) Prof. W. Waldeyer discussed the question, "Does the University of Berlin fulfil the mission entrusted to it by its founder?" but he confined himself mainly to the progress of the anatomical department.

From an analysis published in *Science* for August 4, it may be seen that of the doctorates granted by the United States Universities this year, 32 were for chemistry, 7 for physics, 5 for geology, 4 for palaeontology, 11 for botany, 11 for zoology, 15 for psychology, and so on. "It may be noted that at Johns Hopkins more than half the scientific degrees are given in chemistry. This science also leads at Yale and Harvard. Psychology and education are especially strong at Columbia. Chicago stands first in zoology and in physiology."

According to the Allahabad *Pioneer Mail*, cited in *Nature*, Mr. J. N. Tata's munificent offer to endow a Scientific Research Institute in India has now been dissociated by the generous donor from the proposed family settlement, which was one of the original conditions.

By the will of the late Dr. Jules Maringer, the Pasteur Institute at Paris will receive 100,000 francs.

*Science* reports the following gifts and bequests:—$1000 from Mr. Emerson M'Millin to the research fund of the American Association; about $50,000 to Yale University, by the will of the late Dr. C. J. Stillé; £10,000 to Glasgow University, by the will of the late James Brown Thomson.

We learn from the *American Naturalist* that Columbia University has recently received $10,000, to be known as the Dyckman Fund, the interest of which will be used in the encouragement of biological research on the part of graduate students.

We learn from the *Botanical Gazette* that the extensive botanical library and herbarium accumulated by the late Prof. D. C. Eaton of Yale have been given to the University by his family, and that a graduate scholarship in botany has been founded by his widow.

*Science* publishes the letter in which Prof. C. E. Beecher offers as a gift to the Peabody Museum of Yale University his entire scientific collections, which represent twenty years of personal work, and comprise upwards of one hundred thousand specimens. The collections represent (1) the fauna of the Upper Devonian and Lower Carboniferous in Pennsylvania; (2) the fauna of the Middle Devonian of Western New York; (3) the fauna of the Lower Devonian of Central and Eastern New York; (4) a small series from other geological horizons; (5) about five hundred type specimens. There are hundreds of specimens unique for their perfect preservation and for their careful preparation to show delicate structural details. No other single collection in America is so rich in series, showing the life-histories of species from the embryonic to the adult state.

A course of twelve free lectures on the "Pleistocene Mammals" will be delivered by Dr. Ramsay H. Traquair, F.R.S., in the Lecture Theatre of the Museum of Practical Geology, Jermyn Street, S.W., on Mondays, Wednesdays, and Fridays, at 5 P.M., beginning Monday, October 2, and ending Friday, October 27.

In the *Scientific American* for August 12, Miss Alice Dinsmore gives a lively account of Nature-study in the Summer School of the College of Agriculture of Cornell. There were three departments—the study of insect life, directed by Prof. Comstock; plant life, directed by Prof. Bailey; and farm

life, directed by Prof. Roberts, the instruction in each case being eminently practical. The course is attended chiefly by teachers, and the report gives an impression of sound and thorough work.

The new lecture hall of the American Museum of Natural History is expected to be ready next month. It will seat 1700.

We learn from our esteemed contemporary, the *American Naturalist*, that the state of Wisconsin has appropriated $10,000 for two years for a geological and natural history survey of the state, under the direction of Prof. E. A. Birge. Some of these "appropriations" may be contrasted with, for instance, the apparent impossibility of getting Government support for the survey of Scottish lakes. But the subsidy for the Antarctic Expedition must silence our grumbling in the meantime.

The fine collection of Scottish agates made by the late Prof. Heddle is now arranged in the Museum of Science and Art in Edinburgh. Mr. J. G. Goodchild has prepared a guide to the collection, incorporating Prof. Heddle's explanatory notes.

According to the *Scientific American*, the creation of a great national forestry and game reserve in northern Minnesota, embracing 7,000,000 acres around the headwaters of the Mississippi River, with many lakes of rare beauty, well stocked with fish, will be advocated before Congress next winter by prominent citizens of Chicago and Minnesota. The promoters of the plan are not likely to experience much difficulty in interesting Congress. The game and the virgin forests of the United States are disappearing so rapidly that it is exceedingly important that measures be taken, before it is too late, to save some of the great wooded areas of the continent.

The report of the South African Museum for 1898 by the Director, Mr. W. L. Sclater, gives details as to the growth of the collections. The rocks of the Kimberley mining district have been arranged and displayed, and considerable progress has been made with the collection of South African mammals.

The recently-published British Museum blue-book takes account of the additions to the Natural History collection during 1898, *e.g.* the first instalment of the Norman collection of marine invertebrates, the rare mollusc *Pleurotomaria beyrichii*, the rare fossil Elasmobranch *Squatina alifera*, the late Rev. P. B. Brodie's collection of fossil insect remains, the Piper collection of fossils from the strata of the Ledbury tunnel, and a selection from the late Rev. T. T. Lewis's collection of Old Red Sandstone fossils.

It is noted in *Science* that Dr. A. B. Meyer, the energetic director of the museums in Dresden, is now in the United States inspecting American museums before the new buildings in Dresden are erected.

We learn from the *Victorian Naturalist* that the desirability of removing the National Museum at Melbourne to a more central and accessible site was affirmed at a meeting of the trustees on June 1, and that Professor Baldwin Spencer was appointed honorary director in succession to the late Sir Frederick M'Coy.

It is reported in *Science* that the Boston Public Library will undertake the publication of a card catalogue of physiology with brief abstracts, under the editorship of Prof. W. T. Porter of Harvard Medical School.

We learn from *Nature* that the city of New York has allocated 63,000 dollars for the zoological garden in Bronx Park, and that it is proposed to raise the appropriation for the American Museum of Natural History from 90,000 to 130,000 dollars annually.

It is noted in *Science* that the Ahearn bill, recently passed by the New York legislature, allows $96,000 to be spent next year on free lectures, largely scientific, in New York City.

In *Nature* for August 31, Prof. A. C. Haddon gives an interesting preliminary report on the results of the Cambridge Anthropological Expedition to Torres Straits and Sarawak. He and the other members of the expedition deserve congratulations on the successful issue of their explorations, and the detailed memoirs will be awaited with interest.

The Liverpool expedition for the study of malaria in Sierra Leone included Major Ross and Dr. Annett of the Liverpool School of Tropical Diseases, Mr. E. E. Austen of the British Museum (Natural History) as entomologist, and Dr. S. Van Neck, official delegate of the Belgian Government.

Major Ronald Ross, leader of the expedition, has already succeeded in finding the malaria-bearing mosquito.

It is stated in *Nature* that Mr. J. S. Budgett of Trinity College, Cambridge, has been successful in obtaining eggs and larvæ of *Polypterus*.

Mr. George K. Cherrie has returned from his expedition to Venezuela, where he was collecting birds for the Tring Museum. Although his work was stopped by illness, he got many spoils.

The steamship *Capella* arrived at Tromsö on August 18 from Franz Josef Land, bringing Mr. Wellmann's expedition from Cape Tegetheff. It is reported that the expedition reached the 82nd parallel. Some important scientific work was done and 103 walruses were killed. Mr. Wellmann has been unfortunately crippled by falling into a snow-covered crevasse. He has now returned to Britain.

We learn from the *Botanical Gazette* that Mr. J. N. Rose has just returned from a botanical trip in Mexico, where he rediscovered *Echinocactus parryi*, and collected other species lost or hitherto unknown to American herbaria. He made a thorough study of the species of agave, especially those used in the manufacture of pulque and mescal. He visited, among many other localities, Tequila, in order to find out what plant furnishes "tequila," which is the great mescal drink of Mexico.

The *Scientific American* notes that Dr. Frederick W. True of the Smithsonian Institution, a well-known authority on cetaceans and seals, went in August to Newfoundland to hunt finback whales, in order to obtain specimens for the National Museum at Washington.

The workers on board the Prince of Monaco's *Princess Alice*, which has just returned from a Spitzbergen cruise, were able to do some surveying work, and a large unsuspected bay was discovered. Investigation was much hindered, however, by the vessel running on to rocks, where she remained for five anxious days. Liberation involved a loss of most of the coal, and this forced the Prince to return sooner than he would otherwise have done.

Mr. Benjamin Hoppin has sent his yacht *Senta* to Greenland as a gift to the Peary Relief Expedition, with the sole restriction that he wishes it to be used in scientific exploration.

The *Scientific American* reports that the party of scientific explorers who went to Alaska as the guests of Mr. Harriman, met with success, and made several important discoveries. Among these was an immense bay extending inland for over twenty miles. At the upper end of this bay they discovered a great glacier inferior only to the Muir glacier in size. Several other new glaciers were discovered. Some new plants were found by the botanists, and the collection of marine species is expected to surpass any yet made in northern waters.

We learn from *Science* that Prof. Wm. Libbey and Dr. A. E. Ortmann of Princeton University have gone to dredge and explore in Inglefield Gulf on the steam whaler *Diana*, of the Peary Relief Expedition.

Dr. Robert Logan Jack, late Government Geologist for Queensland, and special commissioner in charge of the exhibits at the Greater Britain Exhibition, has accepted an appointment from Mr. Pritchard Morgan to run some mining concessions in Szechuan, Korea, and North China. Dr. Jack sailed in September.

Dr. J. B. Hatcher, of the Zoological Department of Princeton University, has returned from his expedition to Patagonia, and some account of his results is to be expected shortly in *Science*.

Prof. John B. Smith gives in the *Scientific American* an interesting account of "an improved method of studying underground insects"—by pouring liquid plaster of Paris into the burrows and digging out the cast after it has set. As he says, "concerning the habits of underground insects we are yet greatly in the dark, and much of our supposed knowledge is really inference from observations made upon the insects when at the surface, or from such excavating as has been done in attempting to follow out the burrows of diggers." It may be recalled that the Duke of Argyll used the plaster of Paris method in studying the burrows of the lugworm.

The forty-fourth annual exhibition of the Royal Photographic Society, at the Gallery of the Royal Society of Painters in Water Colours, 5A Pall Mall East, was opened to the public on Monday, September 25, for a period of seven weeks.

The Tsar has recently ordered the allotment of three million roubles to found boarding-houses for University students; and this has been followed by a Government proposal to establish and subsidise boarding-houses, scholarships, and even tutors for the children of the provincial nobility who are attending the middle-class schools.

It is noted in a recent issue of the *Scientific American* that while Britain stands first in the production of slate, and France comes a good second, it will soon be necessary to place the United States well up in the list of competitors. The estimated world production is valued at 16 million dollars, of which $8\frac{1}{3}$ millions fall to Britain, and over $3\frac{1}{2}$ to the States.

The *Times* gives, from a report of the British Consul at Naples, an interesting account of the island of Procida, in the Bay of Naples. Unlike Ischia and Capri, it is very rarely visited by travellers, though in point of scenery it is almost superior to them. It is about two miles long by one mile broad, and carries the enormous population of 14,000 souls. Its sailors are the best in the Bay of Naples, and its little harbour is usually thronged with sailing vessels, which do the coasting trade of Italy and the neighbouring islands. The plateau in the centre of the island produces excellent wine and fruit. Some of the people manufacture very fine gut from silkworms. They call the product *fili di seta*, or "silk threads," the special properties consisting in strength and flexibility. They are made from the stomachs of silkworms just before they begin to spin their silk and form their cocoons. Not many worms in proportion to the amount of gut put on the market are reared in Procida itself, but the makers buy them from Torre dell' Annunziata and other neighbouring towns in great quantities. The worm when fully matured, that is, at the moment when its nourishment ceases, and just before its metamorphosis, is cut open, great care being taken not to injure the membrane of the stomach. This is then removed, and usually reaches the length of 13 to 20 millims., with a diameter of $1\frac{1}{2}$ to 2 millims. The stomachs are then put into a pickle, the secret of

which is carefully kept. When the pickling process is over, the workpeople, who are mostly women, take one end of the stomach in their teeth and draw the other end with their hands. This part of the work requires great dexterity, for the threads are drawn out to the length of 30 to 50 centims., and the whole value of the product depends upon its length in relation to its thickness, and the strain it will carry. There are two seasons for the production—spring, when the best gut is produced, and autumn, when the quality is inferior. There is an important market for this speciality, and the whole production is exported to Northern Italy and abroad at the average price of 150 lire per kilo. The gut is very light, so that a great deal of it goes to a kilo. The cost of production is also considerable, as the worms must be bought just at the moment when they are becoming profitable for making silk, that is when they are at their dearest. Again, the results are frequently disappointing, many worms being found, on dissection, not to be suitable, and having to be discarded.

The *Scientific American* refers to some statistics recently published by the French Meteorological Bureau at Paris. Spain has 3000 hours of sunshine a year; Italy 2700; France 2600; Germany has 1700, while England has but 1400. The average fall of rain in the latter country is greater than that in any other European country. In the northern part and on the high plateaus of Scotland about 351 inches of rain fall a year, and London is said to have an average of 178 rainy days in the year, and fully ten times the quantity of rain that falls on Paris.

In reference to a note which was recently published in our columns on the difficulty of inoculating locusts with fungus owing to the frequent moults, it is interesting to see that the recent experiments at the Cape have proved very effective.

According to Spring's experiments, reported in the *Scientific American* of September 2, a pure blue is the natural colour of water. Finely divided white or colourless particles reflect a yellow light, which unites with the natural blue to form a bright green. The fact that the water of ordinarily green lakes turns perfectly colourless at times, is not due to a clarification, but, on the contrary, to an influx of a reddish mud, coloured by ferric oxide, which completely neutralises the green.

An interesting experiment is being made by the Government of Bosnia and Herzegovina in connection with the subject of the migration of birds. A number of observatories are being established all over these two countries, on the coasts, plains, mountains, rivers, and lakes—in fact, in every spot which seems likely to yield results of interest to those engaged in researches on bird migration. Under the auspices of the Government of the two countries named, a meeting of ornithologists was convened at Sarajevo from the 25th to 29th of September with a view to similar observations conducted on uniform methods being instituted elsewhere. A report was presented on the bird life of the Balkan States, illustrated by a fine collection from those districts.